Chapter 3
Stoneworkers' Approaches to Replicating Prismatic Blades

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3.1 Introduction

The title of this chapter alludes to articles by Don Crabtree (1966, 1968) on the replication of Folsom points and Mesoamerican prismatic blades. I evaluate Crabtree's contributions in light of subsequent experiments in making pressure and percussion blades, with special attention to Mesoamerican blades. I follow a practitioner's perspective in outlining insights gained from attempts to duplicate blades and cores. Jacques Pelegrin (1990: 118) identifies two kinds of knowledge associated with flintknapping: *connaissances* ("knowledge") and *savoir-faires* ("know-how"). Both are gained through knapping and required for knapping. This distinction corresponds to the difference between "declarative and procedural memory" and includes mental representations, programmed knapping gestures, and intuitive and evaluative operations (1990: 118). What must a knapper be able to think, know, and remember in order to follow a sequence of operations and arrive at a desired end? What did ancient blademakers know? The purpose of this chapter is to track the knowledge and wisdom of blademaking proposed by modern knappers.

Tatsuo Kobayashi (1970) proposed a useful distinction for pressure cores. Blades removed from cylindrical cores, such as characteristic of Mesoamerica, belong to his System B. System A blades are from cores made from bifaces, generally for the production of microblades (cf. Inizan 1991; Parry 1994). Most experiments have been with Type B cores. I follow the historic sequence of blademaking experiments for both types of cores, picking up the narrative a century before serious experimentation began. By all accounts, the pivot point was Crabtree's blademaking experiments (1968). I highlight his work and then consider subsequent experiments from knappers around the world. Where feasible, experiments are described in their order of publication. Experimentation with blades has been much more extensive than the

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published record indicates (e.g., Callahan 1985, 1987b, 1995a: 225; Pelegrin 1984b, c; Kelterborn 2008a), but I am limited to publications and written correspondence.

3.2 A History of Mesoamerican Blade Experiments

Descriptions of the manufacture of Mesoamerican fine obsidian blades start with the Spanish Conquest of the Aztecs (a.k.a., *Mexica* or Mexicans) in the early sixteenth century. Eyewitness accounts provide information that has dictated the questions pursued in experiments. One reason the Mexica technique for blademaking was recorded and reported to the Spanish crown was that the ingenuity involved in making "obsidian razors" was proof of the intellectual abilities and skills of the natives and thus constituted a strong Aristotelian argument for the humanity of Amerindians (Titmus and Clark 2003: 90). Modern knappers lament that these accounts do not contain greater detail, but they provide critical details on the manufacturing process and blademaking tool.

3.2.1 The Speculative Prelude to Replication Experiments

To the great early anthropologist Edward B. Tylor (1861), we owe the first contribution concerning Mesoamerican prismatic blades. He published a translation of the description of blademaking found in Juan de Torquemada's (1615) *Monarquía Indiana,* identified artifacts that corresponded to this old description, and also related these ribbons of obsidian to their cores (called "prisms"). His translation of Torquemada [also republished in John Lubbock's *Pre-Historic Times* (1865: 78)] was widely available in Europe (see Daubreé and Roulin 1868: 20–26). Tylor's account of obsidian razors (Fig. 3.1) begins with his visit to an ancient obsidian quarry. His speculations on their manufacture drew on his knowledge of English gunflint manufacture:

> the workman who makes gun-flints could probably make some of the simpler obsidian implements, which were no doubt chipped off in the same way. The section of a gun-flint, with its one side flat for sharpness and the other side ribbed for strength, is one of the characteristics of obsidian knives. That the flint knives of Scandinavia were made by chipping off strips from a mass is proved by the many-sided prisms occasionally found there, and particularly by that one which was discovered just where it had been worked, with the knives chipped off it lying close by, and fitting accurately into their places upon it … One can often see, on the ends of the Scandinavian flint knives, the bruise made by the blow of the hard stone with which they were knocked off … I have heard on good authority, that somewhere in Peru, the Indians still have a way of working obsidian by laying a bone wedge on the surface of a piece and tapping it till the stone cracks. Such a process may have been used in Mexico.
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> (Tylor 1861: 95–99)

Later in his book, Tylor presents the Torquemada account which indicates that blades "were cracked off by pressure" rather than indirect percussion (Tylor 1861: 331).

Tylor's remarkable description started with observations of obsidian blades and related them to artifacts from the Old World. He relied on ethnohistoric accounts, ethnographic analogy to gunflint knappers, fracture mechanics, and a close inspection

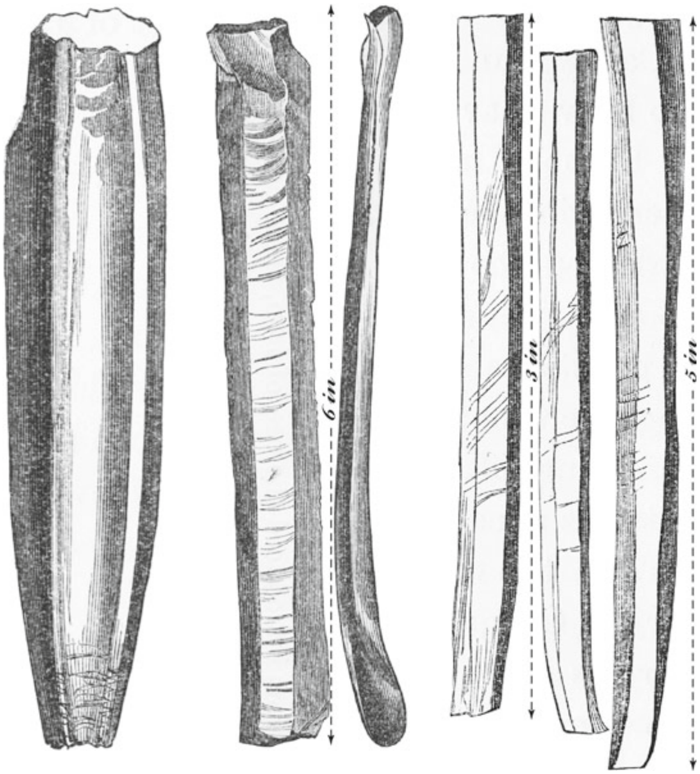

Fig. 3.1 "Fluted Prism of Obsidian," or blade core (*left*), and "Aztec Knives or Razors," or fine blades (*right*), illustrated by Tylor (1861: 96, 98, not to scale)

of cores and blades for the telltale bruises of knapping. It does not seem like much now, but Tylor's identification of prisms as expended cores related to fine blades was a major insight. Four decades later, Adela Breton (1902) still questioned whether these "many-sided objects" were cores, a case made just 2 years earlier by her colleagues William Henry Holmes (1900) and George MacCurdy (1900).

The Torquemada account appears to have been added to Tylor's book as an afterthought. It is not a primary source; Torquemada copied his description of blade-making from Gerónimo de Mendieta's (1971: 406) *Historia Eclesiastica Indiana* (Bk. 4: Chap. 12):

> And they make them [blades], if I can get you to understand, in this manner: they sit on the ground and take a piece of that black stone … That piece which they take is a hand or more long, and thick like the leg or slightly less, and round. They have a staff the thickness of a lance and three cubits long [125 cm] or slightly more, and at the end of this staff they fasten and securely tie a piece of wood one hand long [21 cm], thick like the upper arm or a bit more, and this has its face flat and smoothed, and this serves to make that part weigh more. They put together their unsandaled feet, and with them they press the stone with their chest, and with both hands they take the staff that I said was like a spear shaft, which is also flat and smooth, and they place it against the edge of the face of the stone, which is also smoothed and flat, and then they press towards the chest, and quickly a blade leaps from the stone with its point and two sharp edges …. (my translation)

Tylor also summarized Francisco Hernandez's (1571 [1959]) description of the blademaking tool and interpreted the Latin as describing a crutch-like implement (repeated by Evans 1872: 22; Stevens 1870: 80): "Hernandez … gives a similar account of the process. He compares the wooden instrument used to a cross-bow. It was evidently a **T**-shaped implement, and the workmen held the cross-piece with his two hands against his breast, while the end of the straight stick rested on the stone" (Tylor 1861: 332). This interpretive leap was in error. By remarkable coincidence, however, there is ethnographic support from other Amerindians for a tool like the one Tylor imagined. This information comes from observations of Indians of the Western territories of the United States, as recounted to George Sellers (1886) by George Catlin (quoted in Holmes 1919: 322–323; reprinted in Moorehead 1910:I: 53–55). Catlin's observations significantly influenced the experiments undertaken to make Mesoamerican prismatic blades:

> … good flakes could be split from … [stones by] … *impulsive pressure*, the tool used being a shaft from 2 and 3 inches diameter, varying in length from 30 inches to 4 feet, according to the manner of using them. These shafts were pointed with bone or buck-horn, inserted in the working end as represented in Figure 2, bound with sinews, or rawhide thongs, to prevent splitting. For some kinds of work the bone or horn tips were scraped to a rather blunt point, others with a slightly rounded end of about one-half inch in diameter …. A water-worn pebble broken transversely was commonly held by being sufficiently imbedded in hard earth to prevent its slipping when held by the foot as the pressure was applied. Large blocks of obsidian or any easily flaked stones were held between the feet of the operator while sitting on the ground, the *impulsive pressure* being given to the tool grasped in both hands, a cross-piece on the upper end resting against his chest, the bone end against the stone in a slight indentation, previously prepared, to give the proper angle and to prevent slipping.
>
> In some cases the stone operated on was secured between two pieces or strips of wood like the jaws of a vise, bound together by cords or thongs of rawhide; on these strips the operator would stand as he applied the pressure of his weight by impulse …
>
> Figure 2 [b] represents … the rude sketches made of the flaking tool used to throw off massive flakes, when a sudden percussive pressure was required in addition to the impulsive pressure the man could give. The staffs of these flaking tools were selected from young hard-wood saplings of vigorous growth. A lower branch was utilized, as shown at *a* in Figure 2, to form the crotch in which the blow was struck. Another branch on the opposite side, *a* [prime], was used to secure a heavy stone to give weight and increase the pressure. When the stone to be flaked was firmly held, the point adjusted to give the pressure in the required direction, the staff firmly grasped, the upper end against the chest of the operator, he would throw his weight on it in successive thrusts, and if the flake did not fly off, a man standing opposite would simultaneously with the thrust give a sharp blow with a heavy club represented in the cross section *b* in Figure 2[b], it being so shaped that its force is downward close in the crotch. It has been represented to me that a single blow rarely failed to throw off the flake, frequently the entire depth of the block of stone, sometimes as much as 10 to 12 inches. The tooth or tusk of the walrus was highly prized for tips of the flakers.
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> (Sellers 1886: 874–875, original emphasis; cf. Holmes 1919: 322–323)

After quoting this passage in his comprehensive work on American stone antiquities, Holmes (1919: 323) noted the similarities of Sellers's description to Torquemada's. Sellers's account is more detailed, and it came with illustrations of the tools (Fig. 3.2). The famous illustration of blademaking published by Holmes (1919: 323, Fig. 182) is his own imaginary reconstruction (Fig. 3.3). Holmes's drawing had more impact than the verbal descriptions themselves. There are enough

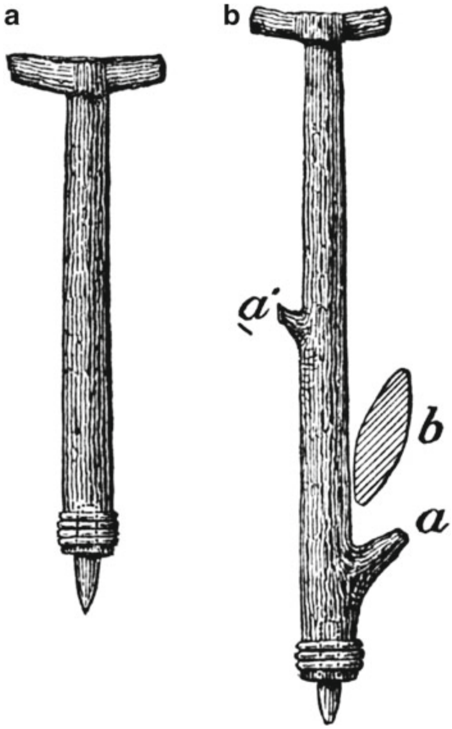

Fig. 3.2 Crutch tools for impulsive pressure and assisted pressure illustrated by Sellers (1886: 874, 875; see also, Moorehead 1910: I:19, Figs. 15, 16): (a) "crotch in which the blow was struck," (a') place to secure a heavy stone for weight, and (b) heavy club (shown in cross-section) to strike a blow

Fig. 3.3 Hypothetical reconstruction proposed by Holmes (1919: 323, Fig. 182) of "Technique of flaking by Mexican Indians as described by Torquemada, and by western United States tribes as described by Catlin"

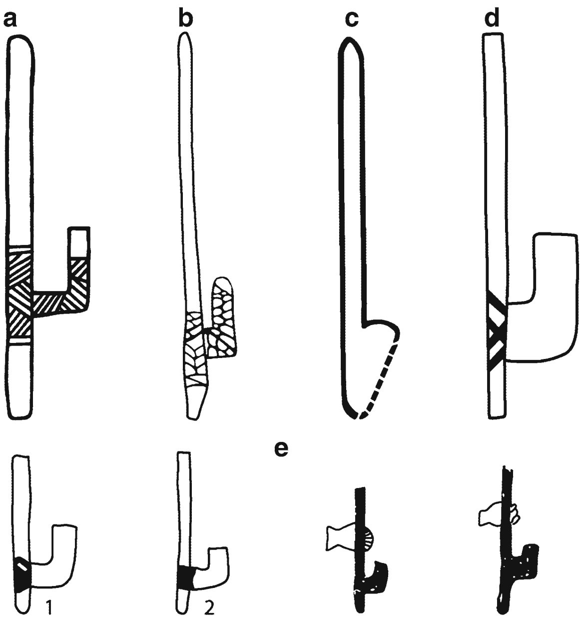

Fig. 3.4 Itzcolotli tools from different sources; note their blunt ends. (**a**) Itzcolotli showing the careful binding of the attached piece of wood to the shaft of a three-cubit-long tool (From Williams and Harvey 1997: 302). (**b**) The tool illustrated by Sahagún (1963: Plate 778) (facsimile, see Thouvenot 1984: Fig. 1 or Clark 1989b: Fig. 1). (**c**) Drawing of the tool from the *Relación de Michoacán* (From Clark 1982: 358, Fig. 1c). (**d**) Ursula Dykerhoff's (1982) drawing of the tool from the *Matrícula de Huexotzinco*. (**e**) Marc Thouvenot's (1984: Figs. 3, 4) depictions of the four tools from the *Matrícula de Huexotzinco* (see also, Prem 1974)

similarities in the descriptions that it appears that ambiguities in Torquemada's summary can be filled in with information from Sellers's account. But combining details from these sources is unwarranted; their amalgamation adversely affected experiments. Different tools and techniques were involved, and these required different working positions, forces, and means of stabilizing cores. Tylor and Holmes assumed the piece of wood tied to the shaft of the Mexican blademaking tool formed a crosspiece of a crutch. It did not. Instead, it formed a hook-like element at one end of a long, staff-like tool called *itzcolotli* (see Clark 1982; Fletcher 1970; Titmus and Clark 2003: 74, Fig. 5.1), as illustrated in Fig. 3.4. The different tools described by Torquemada and Sellers have been the major organizing feature of Mesoamerican blade experiments – a chest crutch versus a hooked lance. Crabtree has long been credited as the first person in modern times to reproduce Mexica razors. An unappreciated irony of his achievement is that he did not make them the Mexican way.

3.2.2 Blade Experiments Before Crabtree

Experiments in lithic technology have been of two types: those designed to understand technology in general and those designed to duplicate specific artifact types. The first starts with experiments and then examines artifacts; the other begins with artifacts and attempts to replicate them. Pelegrin (1991: 120) designates the first approach as "Technical Research." The second approach is known as "Replication Experiments," sensu Jeffrey Flenniken (1981b, 1984), in which an experimenter attempts to duplicate the original conditions and outcomes associated with the manufacture or use of a tool. Historically, the trend has been from Technical to Replication research. Both are valid and useful.

As reported by N. Joly (1883: 212), the first experiments for making Mexican blades were by M. Courtis in 1865:

> M. Courtes [Courtis], member of the French Scientific Commission of Mexico, and M. Chabot, maintain that the Aztecs, in making their obsidian razors, begin by shaping the rock near the quarry whence it was taken. Then after having given to it the form of a prism terminated at one extremity by a blunt point, at the other a flat surface, the workman takes this prism in the left hand, and pressing it against some resisting surface, strikes it at first with light blows, gradually increasing them in force until at last he obtains splinters as sharp as razors, and destined to serve the same purpose.

In his analysis of Aztec blademaking, P. Marcou (1921) provided the 1615 Spanish text of Torquemada and a critique of Tylor's translation of it (see Cabrol and Coutier 1932; Thouvenot 1984). Daubrée and Roulin (1868) had earlier corrected Tylor's translation but did not supply the original text. M. Léon Coutier (in Cabrol and Coutier 1932) tried to follow experimentally the Torquemada description and claimed "it is absolutely impossible for a seated man to effect the abrupt blow that will produce the strong pressure indispensable for detaching a blade" (1932: 580, translation, Olivier de Montmollin). Coutier announced success by indirect percussion instead (see Barnes 1947a: 625). A principal reason for lack of success with the Mexican technique is that Coutier used the wrong tool. Just 2 years before the famous Les Eyzies Lithic Conference in 1964, Coutier demonstrated pressure flaking of flint and glass with a hard wooden tool (Pelegrin 2003: 55), so it was certain that wooden tools could be used on obsidian, as described in Spanish accounts.

A key protagonist in the history of blade experimentation was William Henry Holmes. He described 13 different techniques for chipping stone and many kinds of tools. His coverage defined the limits of the thinkable for most experiments that followed (Holmes 1919). Many tools and holding positions are possible, but fracture was initiated in only four ways: direct percussion, indirect percussion, pressure, and assisted pressure.

The systematic investigation a generation later by William Holmes Ellis at the Ohio Lithic Laboratory, beginning in January of 1938, started with the list of techniques from Holmes's (1919) book and explored them, as well as logical permutations from them. Ellis (1938, 1940, 1965: iv) described and/or tested 19 techniques, including impulsive pressure with a chest crutch, a shoulder crutch, and a “lever and fulcrum” device with a copper tip (Ellis 1965: 36–41). His work was technical research rather than replication, with no attempt to duplicate specific artifacts. Coutier appears to have had some influence on this beginning of the American tradition of formal knapping. Ellis (1965: 9) describes Coutier’s visit in 1937, before the founding of the Lithic Laboratory, and his demonstration to Director Henry C. Shetrone of the wooden billet knapping technique.

Ellis quoted Sellers, Torquemada, and Hernandez (the last two from Tylor’s translations) and depicted pressure work with a T-shaped chest crutch in a manner similar to Holmes’s illustration (Ellis 1965: 37, Fig. XIX). He lists “crutch” in his glossary as a tool with a “bone point inset” (1965: 49). He did not reference blades but implied them under “fluting: channels or grooves in flint caused by the removal of flakes, as from a core or Folsom point” (1965: 49). Ellis apparently did not make any blades by impulsive pressure.

3.2.3 Don Crabtree: Mesoamerican Obsidian Polyhedral Cores and Prismatic Blades

Crabtree started his blade experiments where Ellis left off. He was influenced by Ellis and others, and in his turn, he influenced hundreds directly and thousands indirectly. Crabtree’s accomplishments cannot be separated from his biography. Here I focus on critical points of his life circumstances related to the discovery of Aztec blademaking and related matters. In terms of knapping, Crabtree’s career spanned 50 years and three stages. In piecing together highlights of his career, I relied on published sources and Crabtree’s correspondence, especially that with Jacques Tixier. These letters provide remarkable documentation for Crabtree’s activities leading up to the publication of his papers on Folsom points and Mesoamerican cores and blades.

Crabtree traced his interest in stone tools to when he was 5 years old and “received some arrowpoints for running errands for [his] mother to the neighbors” (Crabtree, in Callahan 1979a: 31). What interested him was how the Indians “were able to work stone harder than steel with such perfection” (in Callahan 1979a: 31). Crabtree’s first attempts at making arrowheads from the local agate were at the age of 7 (Knudson 1978; Times-News 1970), and he taught himself to use hammerstones and billets by the age of 12 (Times-News 1978; Titmus 1981). Against his father’s wishes (Harwood 1999), he kept up this hobby and continued to expand his mastery of different techniques, stone tool types, and raw materials. A trying circumstance arose in 1939 when, at the age of 27, he was diagnosed with cancer. As therapy to recover from cobalt treatments, Crabtree “spent his recuperation period, when his mobility was limited and he was trying to regain muscular strength, flintknapping – making arrowheads, spearpoints, and eccentric forms by the hour” (Knudson 1982: 337). In his convalescence, Crabtree became proficient in making stone tools, and this led to opportunities that brought him to the attention of archaeologists. "In the spring of 1941, fully recovered and with a year of concentrated flintknapping behind him, Crabtree was invited to demonstrate knapping techniques at the American Association of Museums' annual meeting in Columbus, Ohio" (Knudson 1982: 337; see also Crabtree 1966: 6, 22; Titmus 1981; Woods 1981). Contrary to the published account, this followed rather than preceded his 2 months of work in the Lithic Laboratory working with Ellis and Shetrone (letter of R. G. Morgan to Crabtree, March 12, 1941, courtesy of Bradley Lepper). Presumably, Crabtree gained some familiarity with Ellis's (1940) method of experimenting with different techniques and knew of Torquemada's description of blademaking.

Crabtree's budding career was cut short by World War II. During this second stage of his adult life, he helped the war effort, married his wife Evelyn, and worked for a living at a variety of occupations, mostly in the Twin Falls area of southern Idaho. He continued to knap as a serious hobby but did not come to the attention of the scientific community until being rediscovered by Earl Swanson in 1958 (Knudson 1982: 338). A year earlier, Swanson had founded the Anthropology Department at Idaho State College (later changed to Idaho State University) in Pocatello, a city 110 miles on the freeway east of Twin Falls. The Swanson-Crabtree encounter in November of 1958 led to collaboration which lasted until Swanson's untimely death in 1975. Swanson reintroduced Crabtree to the archaeological community by having Crabtree, a craftsman "capable of astonishing excellence in the manufacture of specific kinds of functional stone tools" (Swanson and Butler 1962: 8), give the opening presentation at "The First Conference of Western Archaeologists on Problems of Point Typology" held in Pocatello on March 15, 1962 (Bray et al. 1975: 36; Swanson 1966; Swanson and Butler 1962). "Later that same year, Don suffered a coronary occlusion and was forced to retire from his government position on a disability. However, the participants at the point typology session had become interested in Don's work through Swanson and so with their help [Don] was sent by the National Science Foundation to attend the Lithic Technology Conference in Les Eyzies, France, in November of 1964" (Knudson 1978: 2–3; also Knudson 1982; Smith 1966; Wormington 1985). Retirement allowed Crabtree to dedicate himself almost full-time to flintknapping. Thus began the most productive decade of Crabtree's career. Swanson obtained grant money, mostly from the National Science Foundation, for Crabtree to go to conferences and perform knapping demonstrations, to write articles and a handbook on lithic technology, produce five educational films, and to run a flintknapping fieldschool (ISU Bengal 1965; Knudson 1982; Statham 1978; Swanson 1966). At the time of Swanson's death, he and Crabtree had submitted a sixth NSF grant proposal which requested 2 years of funding for more writing and publication (Swanson and Crabtree 1974). Crabtree intended to explore a block-on-block and a Danish direct percussion technique in making blades; he also intended further studies of cores and blades from Mesoamerica (Swanson and Crabtree 1974: 1–2). Because of Swanson's death, the grant was canceled, and Crabtree never completed his synthesis of blades and blademaking.

The Les Eyzies conference catapulted Crabtree onto the world stage and introduced him to colleagues who became lifelong friends, especially Marie Wormington, François Bordes, Cynthia Irwin Williams, and Jacques Tixier. Each of these scholars influenced and aided Crabtree's career. At the Les Eyzies conference, Crabtree demonstrated the manufacture of obsidian pressure blades and Folsom points (illustrated in Sonneville-Bordes 1967: 50, 51). In turn, he witnessed techniques for making percussion flint blades, a traditional concern of European scholars (see Barnes 1947a, b; Bordes 1947, 1950, 1967). I have not found any concrete record of when Crabtree first made pressure blades, but a 1960 photograph of his display case taken by Gene Titmus shows an Aztec wooden sword edged with obsidian blades (see Times-News 1963, 1978). Crabtree told me it took him 20 years after working with Ellis to figure out how to make blades (Clark 1989a: 131). This would be about the time he met Swanson and Titmus (see below).With encouragement from Tixier, Crabtree started making percussion blades several months after the Les Eyzies conference, and this continued for the next decade (e.g., Bordes and Crabtree 1969b: 1, 7).

Crabtree's blade experiments are best understood within a wider range of activities which included work in fluting Folsom points, a problem that first intrigued him at the age of 16 (Crabtree 1966: 6). His interest in Mesoamerican fluted cores came much later. He described his experimental method more systematically for his Folsom experiments than for his Mesoamerican study. He began trying to make Folsom points in the early 1930s and persisted into the 1970s (Crabtree 1966: 6). Most of these experiments appear to have been casual tinkering rather than formal exercises (see Clark 2002); presumably, many were carried out during his two decades of hobby knapping before 1958. In 1966, he reported 11 broad classes of experiments using different force applications, tools, and holding devices (see the photo-essay in Bray, Swanson, and Farrington 1975). By his own tally, he conducted "hundreds of experiments over a number of years" within these broad classes of techniques (Crabtree 1966: 22). The general classes were (1) direct freehand percussion with hammerstones and billets, (2) direct percussion striking on an anvil, (3) direct percussion with an anvil rest, (4) indirect freehand percussion, (5) indirect percussion with rest, (6) freehand pressure with a short tool, (7) freehand pressure with a longer tool, (8) freehand pressure with a shoulder crutch, (9) pressure with a chest crutch and clamp, (10) pressure with a chest crutch, clamp, and anvil rest, and (11) chest-crutch pressure and indirect percussion with a clamp and anvil (Crabtree 1966: 9). He concluded that Folsom points may have been fluted by direct pressure with rest (10), or by indirect percussion with a clamp and anvil (5), or a combination of the two (11). Much of the work reported for techniques 5 and 11 came from Titmus's collaborative work after 1959, and most of that for technique 10 was solely Crabtree's work (Titmus, 2009, personal communication; cf. Crabtree 1966: 7, 22). Similar techniques were proposed for making Mesoamerican polyhedral cores and prismatic blades.

Crabtree eventually accomplished what Ellis had not, and that was to make fine blades of obsidian using a "chest" crutch, with the core secured in a simple vise. "I suppose one of my greatest thrills in flintworking was the removal of these flakes" (Crabtree, 1979, personal communication). Responding to a question from Errett Callahan (1979b: 11), Crabtree observed: "I think the most challenging [thing] was to understand how a blade was made. That was a great breakthrough – to get a blade with perfectly parallel sides and a trapezoidal cross-section. I think I thought of that for maybe 20 years before I accomplished it. It was far more exciting in a way than the Folsom point." Crabtree mentioned numerous experiments in his landmark study on Mesoamerican cores and blades published in *American Antiquity* (1968), and he demonstrated pressure and direct and indirect percussion techniques in six educational films made from 1968 to 1970 (Bordes and Crabtree 1969a; Crabtree 1969b, 1972a, c–e; Statham 1978; to view, see Lohse 2000).

Crabtree's blade treatise divides into three parts, beginning with an analysis of Torquemada's description, followed by comments on knapping experiments, and ending with results of experiments in high-speed photography for documenting blade manufacture. The high-speed photography began at the end of 1965 (ISU Bengal 1965). Given his purpose to argue for the successful replication of fine blades, it is curious Crabtree started with commentary on Torquemada – nearly equal the space accorded original experiments. Crabtree attempted to reconcile this historic account with his experiments but was ultimately unsuccessful. Observations coming from his experiments made him question Torquemada's account on basic principles. Crabtree claimed to have produced blades by the Mexica technique – *once he modified the technique!* Crabtree observed that if one were to take Torquemada's description of blademaking verbatim, "we have the picture of an Indian sitting flat on the ground, legs straight in front of him, holding a very sharp core between his naked feet, and pressing off blades with a crutch that measures well over 5ft. This simply will not work, and I suggest that the reader convince himself of this by trying this method personally" (Crabtree 1968: 450).

After dealing with Torquemada, Crabtree sketched the parameters of his experiments. Based on years of experience, and through a process of elimination (see Crabtree 1966, 1972a, b: 62; also, Bordes and Crabtree 1969a), he determined that blades and cores made by pressure "have every quality and characteristic of most cores and blades found in Mesoamerica" (Crabtree 1968: 478). In his text, Crabtree alludes to the same variety of experiments described for his Folsom study but does not describe a single one. He made blades with direct handheld percussion using a variety of "percussion tools" (1968: 457). For indirect percussion, he mixed and matched a wide variety of hammers and punches made of hard and soft stone, antler, horn, bone, shell, ivory, and wood; he also reduced different kinds of cores with various holding devices (1968: 459). His experimental percussion cores and blades lacked the regularity and uniformity of archaeological specimens from Mexico, so Crabtree rejected direct and indirect percussion as plausible techniques for making fine blades. He was able to duplicate the final form of Mesoamerican cores and blades, as well as their microcharacteristics, with a technique of applied pressure with a chest crutch. To do so, he found it necessary to immobilize his cores in a wooden vise or clamp, something mentioned by Sellers but not the Spanish friars. For many of his experiments, Crabtree used cores cut from nodules with a diamond saw. Demonstrations of blademaking with such sawn cores became a regular feature of his flintknapping fieldschool, which began in 1969 (Crabtree 1969a).

For his pressure experiments, Crabtree used a simple wooden vise and a chest crutch of semi-flexible wood 32 in. long fitted with bits of copper, antler, ivory, or bone (1968: 452; also, Crabtree 1967: 64–67). He did not mention the number of blades made with different bits or the characteristics of these blades. In demonstrations recorded on film, he uses a copper-tipped chest crutch (see Bordes and Crabtree

1969a; Crabtree 1972e). Most of his blades appear to have been made with the metal-tipped tool, with the focus of his experiments being the reduction of cores of different shapes. Crabtree devoted considerable attention to possible core preforms and the necessity of outfitting them with a straight ridge or two, a bias he learned from Bordes. He described cores of one, two, three, four, and more-than-four starting ridges.

In commenting on Torquemada's description of the blademaking tool, Crabtree alluded to other experiments. One of his most adamant claims was that one cannot hold a core in the feet and push off blades. He experimented with wooden tools and determined that "just a sharpened wooden stick would not be sufficient to make blades" (Crabtree 1968: 449; also Crabtree 1967: 67; Semenov 1964: 59). He speculated that ancient artisans could have used pressure tools with tips of "antler, bone, or jade" – and then he interjected this insightful, parenthetical comment:

> (Since the writing of this paper, I acquired some very hard wood from Mexico … I made an additional experiment of detaching a prismatic blade with a wooden staff minus a metal or antler tip. Because of the limitation of time, I have, to date only removed three blades in this manner. I seated the rounded distal end of the chest crutch directly over a ridge, applied a thrust of downward and outward pressure, and successfully removed several perfect blades. The blades are true replicas.)
>
> (Each time a blade is removed from the core a new position must be selected on the wooden tip, or the tip must be reworked to expose a new surface. In order to remove a blade from a core, the platform must be isolated so that just the platform area of the blade will contact the wooden pressure tip. The tip is not sharp, but it is very blunt in order to give it strength).
>
> (Crabtree 1968: 449)

Crabtree (1968: Fig. 2) illustrated two extremely large blades (7.5 × 20 cm) made by assisted pressure by Titmus and himself. They were produced by Titmus applying all the pressure he could with his copper-tipped chest crutch and Crabtree lightly striking with an antler billet a protruding piece of bolt stock made to simulate a crotch in the tool (Titmus, May 2009, personal communication; Crabtree 1966: 22, 1967: 64), as illustrated by Sellers (1886: Fig. 2). The size and mass of these assisted pressure blades indicate this technique could easily generate sufficient force to produce the largest blades known archaeologically in Mesoamerica. Whether or not large prismatic blades were made in this manner is a different matter (see Hirth 2003b; cf. Fletcher 1970: 212). Crabtree and I discussed alternative techniques for blademaking, and he provided advice on how to proceed (below). One implication of our discussion was that different techniques of making blades may have been involved in Mesoamerica. This was very much a part of Crabtree's thinking (Crabtree 1975b, 1979: 3).

3.2.4 *Jacques Tixier: Flint and Obsidian Pressure Blades*

Tixier and Crabtree met at the 1964 Les Eyzies conference (Jelinek 1965: 278; Smith 1966) and became lifelong friends. Tixier "learned percussion from François

Bordes, and later, when he attended the lithic technology conference in Les Eyzies, he learned the rudiments of pressure flaking from observing François Bordes and me" (Crabtree 1975a: 114). Crabtree demonstrated his technique for making Mexican prismatic blades at this conference, and in 1969, he and Tixier experimented with making percussion and pressure blades during Tixier's visit to Idaho (Tixier 1984b: 58; Tixier et al. 1980: Figs. 15, 20, 21; Times-News 1969; also, Inizan et al. 1992: 662). [Their long-anticipated collaborative research on Capsian blades (Times-News 1969) has not been published (Pelegrin 1984b: 117, 2003; Texier 1982: 59).] Tixier also gained experience in pressure blademaking and fluting bifaces (see Hirth et al. 2003: 150; Tixier et al. 1980: Fig. 31; Inizan et al. 1992: Figs. 33, 42). Tixier became the pivotal scholar in the French tradition of pressure blademaking and analysis (Inizan 1991; Pelegrin 1981a: 5; Otte 1991). As evident in their exchanged letters, starting just a month after the Les Eyzies conference and until their reunion in Idaho, Tixier urged Crabtree to experiment with methods of replicating Capsian blades and cores. They sent each other raw materials and preformed cores so Tixier could make pressure blades of Oregon obsidian and Crabtree could make percussion blades of French flint; Crabtree also sent antler for tools (Crabtree correspondence). In his package of February 23, 1965, Crabtree sent Tixier a large obsidian blade core prepared "with a chest crutch" and small blades "removed by hand pressure" (Crabtree letter to Tixier, February 23, 1965). Tixier sent Crabtree examples of small artifacts for him to replicate and drawings of cores and blades. On November 21, 1966, Tixier sent Crabtree two obsidian pressure blades produced with a copper-tipped crutch he had made, and he also reported having made pressure blades of glass and un-[heat]treated flint (Crabtree correspondence).

Tixier (2000: 2) summarized their collaboration and interchange by modestly downplaying the tremendous impact he had on Crabtree: "I met Don Crabtree for the first time on November 22, 1964, at the 'lithic technology conference at Les Eyzies'. This meeting went almost unnoticed at the time …. Thanks to Don the world of pressure flaking was revealed to us, the French, to the point where many of us, including me, stayed up two nights amazed by Don's ability and knowledge in the production of obsidian tools, the famous knives of the Aztecs, and PaleoIndian projectile points."

Pelegrin (2003: 55) notes that Crabtree's knapping demonstrations at the Les Eyzies conference:

> impressed the small circle of spectators, including the two Frenchmen who subsequently would reconsider the technique in a fundamentally different way. François Bordes, first and foremost a Paleolithic archaeologist, rethought the issue of core-blade pressure reduction in terms of problems of working Old World flint …. Jacques Tixier, however, continued experimentation with Crabtree and, using the chest crutch pressure technique with flint, identified key attributes of pressure blade reduction in several Epipaleolithic industries … Based on these studies, pressure blade reduction of both flint and obsidian was gradually recognized around the Mediterranean by Tixier and his students ….
>
> (Pelegrin 2003: 55; see also Smith 1966: 592)

Tixier experimented with direct pressure, direct percussion, and indirect percussion in making flint blades (1969, 1972; Tixier et al. 1980; Inizan et al. 1992: 15; see his photo-essay of blademaking in Prideaux 1973: 81–91; cf. Newcomer 1975) and identified blades made by different techniques in the archaeological record (1976, 1984b, 1991b; see also Inizan, Roche, and Tixier 1992). This became possible once Tixier isolated some of the distinguishing features of blades made by different techniques. The critical distinction was between pressure and percussion blades. There is considerable overlap between the two types of blades (Tixier 1984a). Tixier (1984b: 66) argued that there was no "key" to distinguishing pressure from percussion blades, but he did identify two clusters of traits: those that occur on pressure blades and those that frequently occur but are not technical stigmata for them. For the first group of attributes, he observed that pressure blades have uniform, parallel edges and ridges that tend to be straight, of a consistent thinness, especially in the mid-part of the blade, have smooth ventral surfaces (lacking ripple or wave marks, especially in the lower part of the blade), and have narrow butts that very rapidly broaden to the maximum width (Inizan et al. 1992: 65). In the second group of frequently occurring attributes, he included short, fairly pronounced bulbs of force, obtuse platform angles, and cores with well-marked flutes from blade removal (Tixier 1984a, b: 66; Inizan et al. 1992: 63; cf. Inizan et al. 1992: 663; Ohnuma 1993: 159; Texier 1984b). These technical stigmata have been widely used to identify pressure blade technologies in the Old World.

As with Crabtree, Tixier's influence has been fundamental for establishing questions and experimental approaches in lithic studies (1980, 1984a, 1991a, b; Otte 1991), clarifying descriptive terminology (Tixier 1963, 1967, 1974; Tixier et al. 1980; also, Inizan et al. 1992; Inizan et al. 1999), and influencing following generations of analysts and replicators.

3.2.5 Payson Sheets and Guy Muto: Obsidian Pressure Blades and Cutting Edge

At Crabtree's 1971 flintknapping fieldschool, and under his direction, Sheets and Muto (1972) reduced a sawn core and analyzed its products and by-products. This teaching exercise was not to evaluate manufacturing techniques per se, but useful information was generated on the types of errors involved in blade manufacture by beginning blademakers and ways of repairing such damage. The purpose of the experiment was to calculate the overall efficiency of Mesoamerican blade technology in terms of the blade length of acute- and obtuse-angle cutting edge produced per gram, a topic of interest to Crabtree and described in one of his films shot 2 years earlier (Crabtree 1972e; Crabtree letter to Tixer, October 23, 1969; see also Bordaz 1970; Callahan 1979a: 30; Crabtree 1971, 1973, 1977; Eren et al. 2008: 952; Leroi-Gourhan 1957). Because of the analysis of experimental products, however, we learn more from this single core than from the hundreds Crabtree reduced but did not analyze. From an 820 gram core with a rectangular platform measuring 6.0 × 5.8 cm, and with the aid of a copper-tipped chest crutch and a vise, Sheets and Muto made 83 blades plus related debitage products. As to efficiency, they calculated 2.3 cm of acute-angle cutting edge per gram of blades, or 2.1 cm per gram of original core weight (Sheets and Muto 1972: 632). They proposed that these ratios of efficiency might allow archaeologists to monitor obsidian scarcity in prehistoric times. Pressure blades, of course, are much more efficient than small percussion blades.

> For instance, in 1971 Don Crabtree removed a series of 12 percussion blades (cutting edge length 258.4 cm, 385.1 g) from an obsidian core, yielding a CE/M [cutting edge per mass] ratio of 0.74. The CE/M ratio of these blades individually increases markedly with the order of their removal. The earlier blades are relatively thick and irregular, and were removed more for the purpose of shaping the core.
>
> (Sheets 1978: 46–47)

Of interest in this fieldschool exercise, Sheets and Muto removed two blades with a blunt, hardwood tool, a repetition of Crabtree's last-minute experiment in 1967. "The blades removed with the wooden tool had small and diffuse bulbs with accentuated lips, and no 'eraillure' (bulbar) scars. Blades removed with the chest crutch [with a copper bit] had larger bulbs, smaller lips, and more eraillure scars" (Sheets and Muto 1972: 632).

Concern with economic efficiencies brought into question the features of prismatic blades under artisan control. For the analysis of cutting-edge efficiency, these are blade length, width, thickness, and cross section morphology.

> The maximizing blades, those carrying a maximum of length for their weight, are relatively thin and narrow. Thickness and width are controlled by the knapper, within limits. Thicker prismatic blades are produced by seating the tip of the pressure tool farther from the edge of the core …. Manufacturing thicker blades does require more force, so if Crabtree's technique (1968) using a chest crutch and the knapper's weight was used, that weight sets a limit to the thickness and width of blades produced.
>
> (Sheets 1978: 44–45)

Crabtree was inconsistent in his opinion on the role of body weight in making pressure blades. In his principal paper, he observed that all the participants at Les Eyzies were able to detach blades, but the largest blade was made by the smallest person there, Denise de Sonneville-Bordes (Crabtree 1968: 451; cf. Jelinek 1965: 278). The same lesson was repeated at his 1969 fieldschool where Lucy Lewis made some of the best pressure blades (Crabtree 1969a: 5). But Crabtree and his male students kept coming back to body weight as a parameter limiting the size of blades that could be made. Crabtree (1968: 468) records: "I have removed blades 1 in. wide and 8 in. long by the use of the pressure crutch alone, and yet my total weight is only 165 pounds, which makes it impossible for me to exceed this much downward pressure." In contrast, he claimed to have been able to exert a force of 300 pounds through a shoulder crutch (Crabtree 1967: 68).

3.2.6 J. B. Sollberger and Leland W. Patterson: Flint Prismatic Blades and Microblades

Sollberger and Patterson published two series of experiments, with the first focused on the characteristics of flint blades made with different techniques and a follow-up replication experiment for duplicating Iranian microblades. They called their first exercises "replication" experiments, but this is a misnomer because they did not have a replication target in mind (Sollberger and Patterson 1976a).

Sollberger was another American original along the lines of Crabtree, with a special genius for inventing devices to help in flintknapping (Callahan 1995c; Harwood 1987). Sollberger and Patterson were both knappers and part of the knap-in scene of the 1980s, and both dominated the pages of *Flintknappers' Exchange* and *Lithic Technology* with ideas, criticisms, and knapping tips. Patterson (1988) learned from Sollberger, a self-taught knapper. Sollberger's interest in archaeology long preceded his subsequent knapping career. He was involved in Texas archaeology as an amateur and began writing site reports by the late 1940s (Sollberger 1948, 1949). He began knapping in the 1950s but could not get things to work. As Patterson (1988: 20) reports, "Solly once said he got started in flintknapping after watching someone make a simple unifacial scraper, and once he had the 'bug', there was no stopping his drive to become one of the best flintknappers of this era." Sollberger would not claim this status for his own work, but it is an appropriate attribution. His earliest attempt involved a forked stick for a holding device and the use of a copper lever to make a flint dart point (Sollberger 1978: 6; also Harwood 1987). He started serious experimentation about 1967 and was influenced by Crabtree's (1966) publication on Folsom points (Callahan 1978c: 13).

As did Crabtree, Sollberger started by making arrowheads and bifaces (1967, 1968, 1969, 1971, 1976; Sollberger and Patterson 1980), moved on to fluted points (1977, 1978, 1985, 1988, 1989), and then made blades with some of the same techniques used for fluting (Sollberger and Patterson 1976a, 1983). For his part, Patterson began publishing in 1973 and appears to have hooked up with Sollberger about that time; they co-published their first article the next year (Patterson and Sollberger 1974). Their focus on blades came from Patterson's work and interests (1973, 1976, 1981, 1986). Their partnership was a mutually beneficial division of labor in which Sollberger was the knapper, experimenter, and fracture theorist (Callahan 1995b, 1996b; Sollberger 1993; Sollberger and Patterson 1976a, b, 1980, 1983) and Patterson the writer and principal analyst (Patterson 1982, 1990; Patterson and Sollberger 1974, 1978, 1980).

In their major co-paper on flint blades, nine experiments were conducted to discover differences in the attributes of the resultant blades. Two experiments were direct percussion with a hard hammer, another two with a soft hammer, three with indirect percussion (using a wooden vise to hold the core), and two with pressure using an antler. For one experiment, they immobilized the core in a fork-like device (Fig. 3.5a). For the other, the core was handheld (Sollberger and Patterson 1976a: 524). They found that many features on blades were not diagnostic. As with Bordes and Crabtree's (1969b) study, Sollberger and Patterson assessed blade attributes for populations of blades made by different techniques rather than for individual specimens. With his approval, they also corrected Crabtree's speculation concerning the need to create straight ridges on core preforms to make blades. Making a guiding ridge was not necessary (Sollberger and Patterson 1967a: 525).

Sollberger and Patterson's later experiment began with a consideration of microblades (i.e., less than 11 mm wide) and cores from Iran and attempted to duplicate

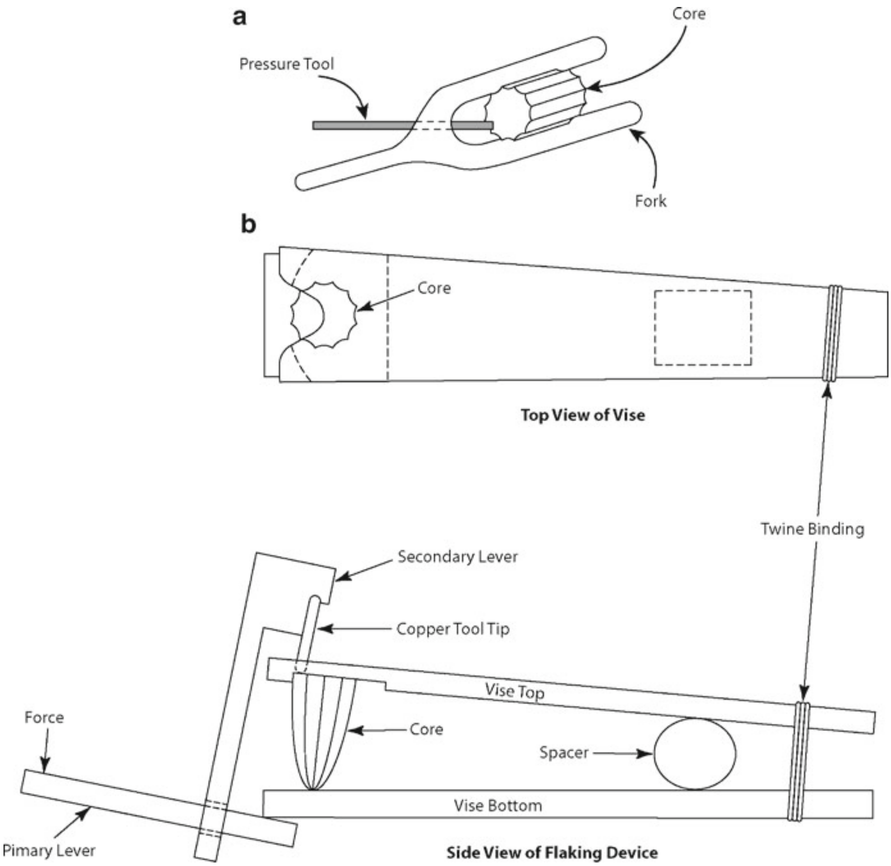

Fig. 3.5 Holding devices used by J. B. Sollberger. (**a**) Forked device to be used with a lever (Redrawn from Sollberger and Patterson 1976a: 524, Fig. 4). (**b**) Vertical clamp for lever pressure (Redrawn from Sollberger and Patterson 1983: 26, Fig. 1)

their features. For these experiments, they used a clever device similar to that employed by Sollberger to flute Folsom points by lever pressure (Fig. 3.5b). It is a combination of a vise with vertical jaws and a lever tool with a copper tip (Sollberger and Patterson 1983: 26, Fig. 1; cf. Sollberger 1978, 1985; for photographs of Solly using this tool, see *Flintknappers' Exchange* [1980] vol. 3: no. 2:16, no. 3:7–8). The tool (Fig. 3.2) is similar in conception to a technique of flaking illustrated by Sellers (Fig. 3.6). Sollberger's device immobilized even small cores without stressing the part of the core platform and face being worked, and it also permitted the slow application of pressure until fracture initiation was achieved. Consequently, stress to cores and blades was minimized, as were manufacturing failures (Sollberger and Patterson 1983: 27). This is really a hybrid technique, half machine, half human. In this experiment, Sollberger and Patterson duplicated their observation about the nonnecessity of preparing an initial ridge on blade cores (1983: 27). They also

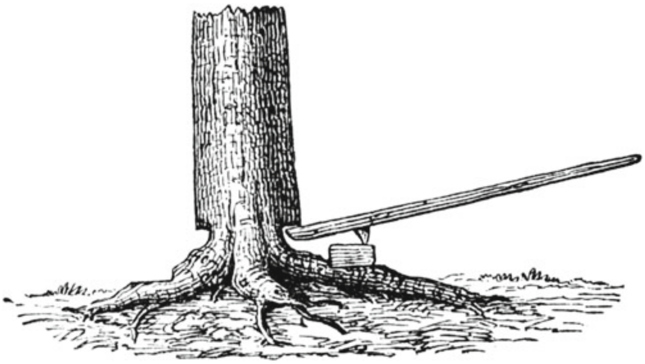

Fig. 3.6 Lever pressure used in conjunction with a notched tree (Redrawn from Sellers 1886: 883, Fig. 7; see also, Moorehead 1910: I:21, Fig. 19)

stressed the need to abrade the core platform to reduce tool slippage and of the critical feature of the angle of the platform to the core face. They found it difficult to produce blades if this angle slightly exceeded 90°. They found that blades with their lever-vise were consistently longer than blades made by direct freehand pressure (Sollberger and Patterson 1983: 29).

3.2.7 John Clark: Mesoamerican Obsidian Cores and Blades

My knapping education consisted of watching two old men pressure flake long spearheads from red glass at a scout jamboree when I was about fifteen, studying soon after the photo-essay of Bordes making tools by percussion in the book *Early Man* (Howell 1965: 118–119), years later studying the photo-essays of Tixier making flint blades by direct and indirect percussion in the book *Cro-Magnon Man* (Prideaux 1973: 83–91), reading Crabtree's articles as a college junior in 1975, and viewing several of his films in a class on primitive technology. The film *The Hunter's Edge* impressed me, but I did not internalize details of blademaking beyond the fact that it depended on a vise and a chest crutch. My knapping started about 1967 with making arrowheads from window glass and bottle bottoms with nails, then moving on to obsidian and percussion with a small antler billet.

Necessity motivated me to learn of Mesoamerican blades in 1977 when I was hired to work in southern Mexico on an obsidian assemblage from an early city there. This began my active period of analysis and experimentation, which lasted until I started dissertation research in 1984. I read Sheets's work in 1977 and reread Crabtree's articles, and these guided my course well before I met these scholars in person. I wanted to replicate blades but found I could not get started. I introduced myself via a letter to Sheets and asked for advice. Mail back and forth from Mexico to the United States in those days was glacial, so it was some time before Sheets's helpful reply got me on track. In the meantime, I studied collections of obsidian artifacts.

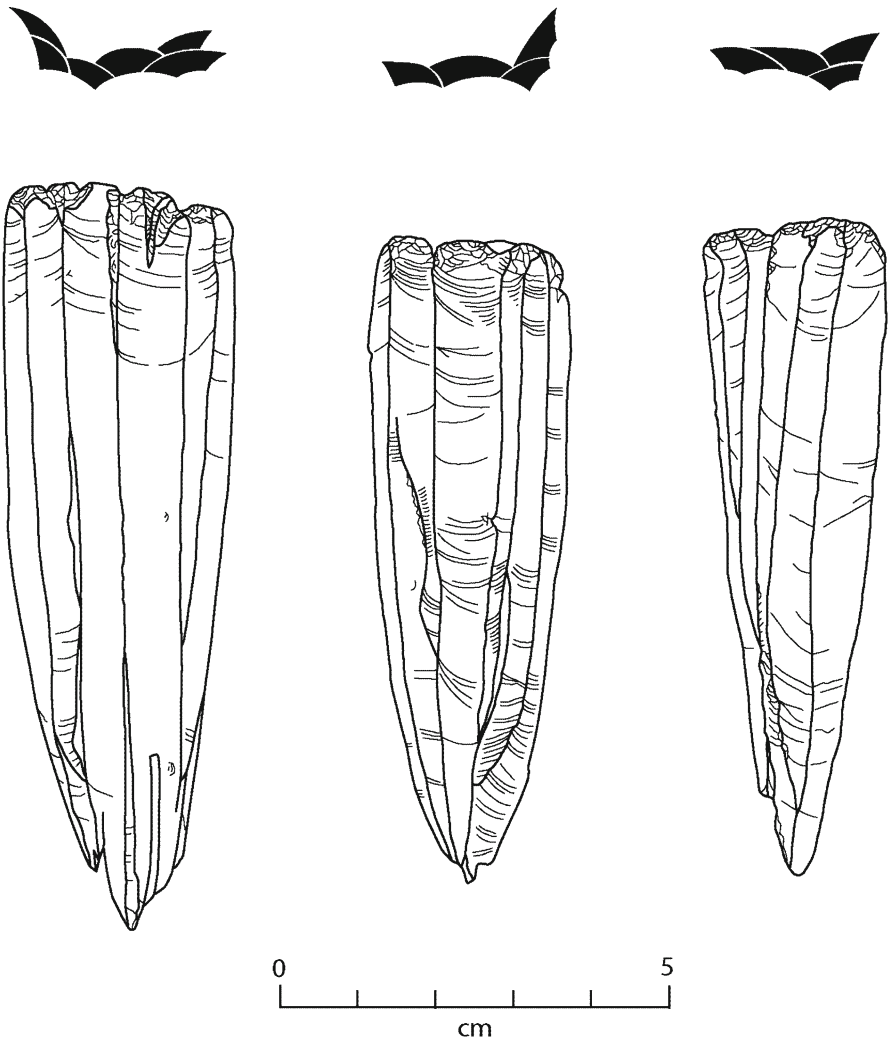

Fig. 3.7 Refitted blades and partial core shells from the Late Classic Maya site of Lagartero, Chiapas. The black silhouettes above each core shell show blade cross-sections and sequences of removal

My first detailed study (unpublished) probably had more impact on me than I have realized. Susanna Ekholm allowed me to study a cache of 73 whole prismatic blades from the Late Classic Maya site of Lagartero (in Matheny 1988; see also Ekholm 1979). I sorted these blades into their original 11+ cores and refitted partial core shells for the interior ring of three different cores (Fig. 3.7). I was fortunate to study a collection of nine percussion cores at a regional museum that corresponded to two stages in Sheets's (1975) behavioral model for blade manufacture (Clark 1977a, b), and these provided a concrete idea of what core preforms looked like. So, over a

4-month period, I became familiar with the ending and beginning stages of pressure blade cores. I had also started to teach myself blademaking, beginning with the thick base of a broken water bottle and finally obtaining obsidian from Guatemala.

I could not follow the knapping tips in Crabtree's (1968) article because I could not hold my percussion cores still. By this time, Sheets had sent me a photograph of the vise used at the 1971 Crabtree fieldschool. With this visual guide, I was able to make a suitable vise, secure my cores, and move into production. Working in primitive conditions, I had no possibility of cutting out cores with a diamond saw, so I made them from scratch by direct hammerstone percussion. With a chest crutch and a vise, I managed to reduce these cores and produce fine blades suitable for use-wear experiments – the main objective of my blademaking.

Because I started from scratch, I learned about the percussion end of the reduction process and of the transition from percussion to pressure. Following Sheets and Muto's (1972) experiment, I classified and analyzed the by-products of reduced nodules all the way down to the pressure blades and expended cores. This was done as part of my Master's thesis. By the time I finished it (Clark 1979), I had met and conversed with Crabtree several times about his technique (Clark 1989a). In writing up the blademaking process for my thesis, I explained how Crabtree had rediscovered the pressure technique for blademaking by following Torquemada's description. But when I completed the comparison, there was scant correspondence between Crabtree's and the Mexica techniques (Clark 1979: 245–253). Critiques of Crabtree's handling of the Spanish sources (Feldman 1971; Fletcher 1970) had already clarified that the Mexica used a different tool than the chest crutch postulated by Tylor, Holmes, Ellis, and Crabtree.

Some of my questioning of Crabtree's reconstruction of the Mexica technique derived from an article in which Sheets (1977) outlined pending questions and opportunities for Mesoamerican lithic studies. He argued that "Coutier's consideration of the possibility of using indirect percussion for blade manufacture deserves more analytic and replicative attention than it has received … replication of core-blade technology might also consider the possibility of *pulling* the pressure blades off rather than pressing" (Sheets 1977: 143–144, original emphasis). I corresponded with Sheets about his "pulling" technique, and he informed me that it was still a hypothetical possibility rather than a verified procedure: "Let me clarify something. Nowhere have I said that I have pulled prismatic blades off, for I have yet to try it from start to finish. I am pretty sure that it can be done …. If the core were held between the feet, tip toward the worker, and a notched or hooked stick used, then the much greater strength of the body could be used to exert much greater force than the simple weight of the body" (Sheets's letter of September 9, 1978).

The following year, I was hooked on the blade problem. I began my second encounter with obsidian blade technology by making a tool like the one illustrated by Charles Fletcher (1970; see Fig. 3.4b). I took the proportions of his illustrated tool as accurate and converted them to my size by calculating my cubit length as 50 cm and making a tool three cubits long (Clark 1982: 366). I knew nothing about the tool or how it might work, or the adequacy of the descriptions. I put a copper tip in the end of the hook part of this implement because I knew copper bits worked for blademaking. My question was whether someone could sit down, hold a core with naked feet, and press off blades with the tool Fletcher illustrated from Bernardino de Sahagún (1963). I began by testing the tool, core position, and the physics of the procedure as I imagined it. I related my earliest experiments in a letter to Crabtree (October 8, 1979):

> I put the core upside down in the vise, the core facing me, just to see if the mechanics would work … and then I pulled off a blade in the vise, so there was a possibility that the mechanics would work OK, but then my next problem was to figure out how to hold the thing with my feet, and so I started on this and had all kinds of problems. I destroyed my core; I went back in and put it in the vise and used the chest crutch and fixed my core and went out and tried it again, and I started pulling off some blades.

Both Crabtree and Sheets pointed out to me that I had not really "pulled" blades off the core but must have pushed them off if the core was in front of me, with the platform facing me.

These simple experiments were "tinkering," akin to those described by Crabtree. Their focus was to evaluate the reliability of information provided in the Spanish descriptions. I was concerned with the performance characteristics of the manufacturing tool, the material used for the bit, and whether or not one could make blades in a seated position. I verified that blades could be made with this tool while in a seated position (Clark 1982: Figs. 8, 9). My major concern at this point was for the working end of the blademaking tool. I experimented with tropical hardwoods and with oak from a pick handle. I made several blades with a tool of oak wood, but the process of blademaking compacted the wood, so the bit required constant maintenance (see Clark 1982: 371).

The principal conclusion of these first experiments was that the technique described by Torquemada was feasible in all aspects I could check. Coutier (Cabrol and Coutier 1932) and Crabtree (1968) both pronounced Torquemada's description "impossible," but they had used the wrong tool. Sheets proposed that the "pressing towards the chest" mentioned by Mendieta and Torquemada could be a description of a "pulling" motion involved with a lever tool. This suggested to me that the wooden hook of the itzcolotli was the working part of this tool. I am not certain of this (Titmus and Clark 2003: 77, 87, 96), but it is my best guess. The tool certainly functions well in this manner.

By the time I started experimenting with an itzcolotli, I had been making blades the Crabtree/Catlin way – or so I supposed – for 2 years. I measured the working angle used with the chest crutch and tried to design an itzcolotli so I could duplicate the same angles while sitting and holding a core with my feet. Knowing the angle was important for deciding how to position the core on the ground. Most details of my beginning attempts with an itzcolotli are now fuzzy. I could not get the thing to work with a pulling or pressing motion, so I resorted to more of a pushing motion. I remember clearly one occasion when I placed the copper tip of my tool on the core platform and pushed/pulled without much force, and a perfect blade lifted up a few millimeters and then settled down again on the newly created flute on the core. I was astounded. This blade required minimal force, and it ran all the way to the end of the core.

Another learning experience came at the Pachuca Obsidian Conference in January 1981. At this conference, we visited obsidian quarries, examined collections, heard papers on the full range of obsidian topics, and witnessed the Catlin and Mexica techniques side-by-side (Clark et al. 1981, 1982). The memorable experience for me was watching Titmus and Flenniken make blades. In trying to fabricate a core among us, it was obvious to all how difficult percussion preforming of a pressure core could be (see Crabtree 1968: 446, 451; Inizan et al. 1992: 24; Pelegrin 1984b: 126; Titmus and Clark 2003: 91, 93; cf. Bordes and Crabtree 1969b: 3). I made a large conical core by direct percussion, and we finally got it locked tight in Titmus's vise. Both Flenniken and Titmus attempted to remove blades from this core but were unsuccessful. Neither Flenniken nor Titmus was used to making blades from a scratch core, and this was a source of difficulty. I watched Flenniken set up the tool and push and strain to no avail and then give way to Titmus for more of the same. With the core secured in the vise, Titmus stood slightly behind the core and put a chair in front of him to his left. He placed the tip of his chest crutch on the outer edge of the core platform at about a 75° angle, positioned the crosspiece of the crutch on his chest, and held the shaft of the crutch tool with his right hand – while holding onto the back of the chair with his left hand to steady himself (cf. Crabtree 1972e). The reason for holding onto the chair was that he knew from watching Flenniken's attempts that he would need to exert tremendous force to remove a blade, and he was guarding against falling down once the blade broke free.

As it turned out, this precaution proved unnecessary because Titmus did not press off a blade either. Apparently, the first blades off a percussion-preformed core require more force than later ones. I watched these attempts carefully because I had never seen anyone make pressure blades live. It was now my turn to try Titmus's tool. Before attempting a blade removal, I abraded the core platform with my sandstone hammerstone and wiped away the dust. I heard concerned comments from Flenniken and Titmus that the dust would be useful to keep the tool from slipping (supported by Callahan 1985: 36); I removed it for the same reason they advised keeping it. I also set up on the core on the opposite side favored by them. With the core embedded vertically in the lateral jaws of the vise, and with sufficient weight placed on the back end of the vise to prevent the whole contraption from moving, I stood slightly in front of the core, with my back to it, and extended the tip of the chest crutch between my legs and behind me to reach the leading edge of the core platform. The angle of the shaft of the crutch to the plane of the platform was about 105°. Again I heard from Flenniken and Titmus that I was courting disaster.

Standing in front of the core and reaching back to the core platform, I seated the copper bit of the chest crutch between two ridges on the platform to make a trapezoidal blade. Bending over to accommodate myself to Titmus's chest crutch, I locked my elbows over my knees and then bent my knees forward, thereby dropping my weight on the tool at the same time I pushed it forward with both hands. The result was a massive, first series blade that ran the length of the core. I did this several times in short order to take off a series of blades on the portion of the core platform arc exposed in the vise. We then repositioned the core, and Flenniken and Titmus made blades in the same manner. They found that the change in body position and the obtuse tool-to-core angle required less force than they were accustomed to expending. Among us, we took several rings (or series) of blades from this large core, at which point it had become a straight-sided and regularly fluted core – the form Crabtree imagined as the starting core for pressure.

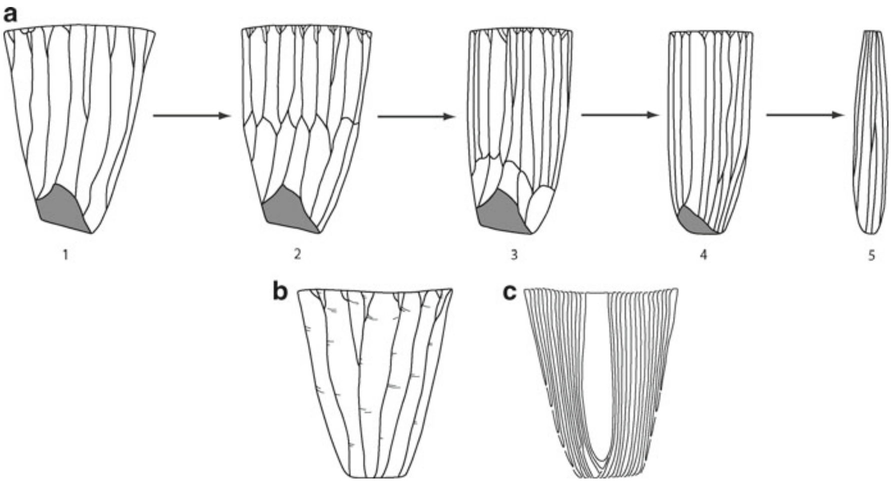

Fig. 3.8 Reduced blade core. (**a**) Sequential changes in the original core. (**b**) The original profile of the core. (**c**) Core and blades from successive rings shown in profile (Redrawn from Clark 1997: 138, 139, Figs. 2, 3)

One of the peripheral benefits of the Pachuca Conference was that Flenniken and Titmus left their chest crutches and vise in Mexico for my use. After returning to southern Mexico, I had the following experience (letter to Titmus, April 23, 1981):

> I did a little biface and blademaking just after I got back …. As I told Jeff … I took the small chest crutch that he gave me and tried to duplicate the Catlin (in Sellers 1886) technique. [Sellers] actually describes a chest crutch and sitting down and holding the core with the feet. I used pretty much the same angles that I described in my blade paper and that I used in Pachuca with the chest crutch. I had all kinds of success with this technique. It is not even necessary to take off your shoes if you don't want to. In my first reduction of an entire core I produced 290 blades in less than two hours – and that was without hurrying and also stopping to remove hinge fractures, etc [shown in Fig. 3.8]. Needless to say, I was stunned. I still don't know what to think. I don't want to accept it. Perhaps the biggest advantage that I had was the ground platform. It was one of your cores. It is a bit more difficult if there is minor topography on the platform and if it isn't all ground down.

Because of differences in our heights, Flenniken's chest crutch fit me as a stomach crutch, and having a comfortable tool clearly was part of the success of this experiment. I was able to reposition the core in its slight depression with my feet during blading and thereby maintain a rhythm of motion and force in blade removals. This was a new experience for me.

My limited experiences in blading were uncomplicated. As were Crabtree's (1968) experiments, mine were *feasibility exercises* to determine whether or not something could be done – or more precisely, whether I personally could achieve some knapping outcome. The questions I wondered about came from Spanish descriptions of the blademaking technique. One of the last series of verification experiments I was involved in was to check the possibilities of bits for the pressure tool. Some artifacts I was studying at the time suggested to me that some bits may have been made of slate. For the same archaeological collection, I identified stone punches used in indirect percussion that were similar in form, size, and wear patterns to antler punches still being used by the Lacandon Indians of Chiapas, Mexico (Clark 1985, 1988). One punch-like tool, however, lacked the wear on its tip expected had it been used as a punch, so I entertained the notion that it might have been a bit for a pressure tool. I tested a bit made of the same kind of stone as the artifact under study, and I also made the occasion to test bits of shell, bone, chert, obsidian, and tropical hardwood. For experimental control, I employed a chest crutch and vise so I could monitor one variable at a time, namely, the material of the bit. All materials tested served to produce fine blades. I was also able to use these tools to remove blades from foot-held cores (Clark 1985). I observed some differences in the platform characteristics of blades made with wooden bits versus those of stone, but I tabled this observation for later (see Titmus and Clark 2003: 89). After I published these experiments, I next tried to use these same bits in itzcolotli tools deployed while in a seated position and foot-holding a core. I was unsuccessful in these attempts. I do not remember producing anything that would qualify as a fine blade. These negative outcomes suggested to me that bits of these other materials require more force to use than do copper ones.

My intent was to monitor one variable at a time, but this was not possible because the morphology of each tool tip was significantly different for the different raw materials, and some of these differences had significant effects. Using a bit of tropical hardwood (granadillo) in a chest crutch was much more effective than using it for the tip of the itzcolotli had been years earlier. I did not have to resharpen it to remove compacted facets.

The blades made with these wooden bits are the largest blades I have yet made … Some are over 30 mm wide, several millimeters thick, and 17 cm long. To make these large blades, I used a vise to immobilize the core … the wooden bit was attached to a chest crutch for experimental control. I held the chest crutch with my left hand, at the same time pressing with my chest. Just after this pressure was exerted, I hit the bottom of the chest crutch, near the bit, with my free right hand – thus forcing off a large blade …. The blades produced in this manner show pronounced lipping, more so than those produced when only pressure is used (Clark 1985: 1–2; cf. Pelegrin 1984b: 118).

The bit made of chert had a much sharper point. “Most of the blades produced with this tool differ from blades made with other bits; obvious Hertzian cones are visible on the bulbs of pressure, and the eraillure scars are more complex on blades produced with the chert bit” (Clark 1985: 4). This may be because of the raw material and/or its sharper point.

3.2.8 Pierre-Jean Texier: Pressure Blademaking and Fracture Mechanics

Judging from publications, the early 1980s witnessed a resurgence in France of experimentation in making pressure blades, with a major event being the blade symposium put together by Tixier in 1982 (*Préhistoire de la Pierre Taillée: 2 Économie du*

Débitage Laminaire, 1984). Two years earlier, Texier reported a simple experiment in making blades from a long, tabular core with a chest crutch, a technique learned from Tixier (see Tixier et al. 1980: Figs. 20, 21). These narrow, tabular cores mimic Type A cores (Kobayashi 1970). The purpose of Texier's experiments appears to have been to understand fracture mechanics rather than techniques for making blades (1984a, b; see Gallet and Texier 1991). Texier described the effect of the transverse arc of the core face in terms of making blades of different thicknesses. Narrower cores produced fewer blades, and narrower blades, per arc than wider cores (1982: Fig. 1).

Many of Texier's observations are apropos to current discussion. He (1982: 58) argued that the characteristics of the butt ends of blades had more to do with the mode of force application (e.g., direct percussion) than the means by which it was delivered (e.g., hammerstone versus billet). His experiments with narrow and wide cores (i.e., Types A and B) led to the observation that narrower blades required less force to remove than wider ones (if one holds thickness equal) and that sharp indentors focus the force application; thus, for fine raw materials such as obsidian, one should work with blunter tools to avoid crushing the platform (1982: 63). As later argued by Pelegrin (below), Texier suggested that large blades implicate a different set of parameters than do the medium-sized blades made in his experiments. The purpose of his experiments was to assess differences between percussion and pressure blademaking. Because of the necessary controls involved in pressure blademaking, such as immobilizing cores in vises, ancient artisans were able to do much more precise work, and with a greatly reduced error rate (1982: 64).

3.2.9 Bo Madsen: Danish Flint Blades

Madsen is the acknowledged master knapper in Denmark and has long been involved in numerous experiments in making flint tools (Madsen 1984, 1989; Hansen and Madsen 1983), with his flint blade experiments going back to at least 1979 and the famous Lejre Seminar (see Callahan 1980a). Madsen became interested in flint tools as a boy and wondered how they were made. He started doing percussion work at the age of 15 when he was a volunteer at the Kulturhistorisk Museum. He made his first percussion blades at 19 (September 1, 2009, personal communication). In 1971, he came across publications by Bordes and Crabtree that got him going. Because of language barriers and technical jargon, it took him several months to work through Crabtree's (1966) paper on Folsom points. As a student, he met and worked with Bordes at a knapping demonstration in 1973 (September 1, 2009, personal communication). In 1975, Madsen met Jacques Pelegrin, and the two have been knapping together ever since. Both worked with Bordes for a month in 1977 and were inspired by him. Madsen started serious work on blades after that time (Callahan 1980a: 20, 1980b: 25, Fig. 1). Madsen met Callahan in 1979 at Lejre and worked with him in arranging the 1979 and 1981 seminars (September 1, 2009, personal communication). He was not satisfied with his blade work at that time (Callahan 1980b: 23) and has continued to work on it (Madsen 1992, 1996).

Results of the 1979 conference are of interest. Twelve knappers of varying ability experimented with different technologies, and also in the creation of an archaeological site (Madsen 1981). Ten to twelve different techniques were demonstrated on how to make blades, and then the participants went to work. The objective of their experiments was to see whether they could define the technical stigmata for direct percussion blades made by stone hammers versus those made with antler billets. In particular, seminar participants wanted to replicate the characteristics of blades from the site of Trollesgave in southern Zealand, Denmark (cf. Fischer 1990).

> With this template in mind, four experimenters each made a minimum of 30 blades with a soft hammerstone; another series of experiments using red deer billets … was also conducted. The experimenters used direct percussion, holding the core either freely in one hand, on the thigh, or in one case on the ground. Hereby a "population" of more than 250 blades was produced in addition to interesting cores, fragments and flakes.
>
> (Madsen 1981: 17)

A total of 601 documented blades were produced and analyzed for over 30 features such as blade curvature and the type of overhang removal (Callahan 1980d: 4; Madsen 1981: 18). Perhaps not surprisingly, there was no clear division among the characteristics on blades that corresponded to the fabricators involved. Rather, the attributes varied along a continuum between the two depending on the elasticity of the fabricator (see Madsen 1981: 18, Fig. 4; below). Madsen has since performed more experiments and used the results as guides for reconstructing the techniques and methods of blademaking at different sites (Madsen 1992, 1996). He considered 28 attributes of blade platforms and provided a useful illustration of them (see Madsen 1996: 69, Fig. 5). The Denmark experiments are valuable as a model of how to conduct experiments and how to analyze the output of experiments.

3.2.10 Katsuhiko Ohnuma: Detaching Microblades

Ohnuma (1993) conducted a significant comparative study of microblades from Iraq and Japan that relied on experimental outcomes to interpret archaeological specimens. His microblade experiments were patterned after earlier experiments carried out with Bergman and Mark Newcomer to distinguish flakes and blades made by hard- and soft-hammer percussion (Ohnuma and Bergman 1983). Ohnuma and Bergman learned basic knapping skills from Newcomer at the Institute of Archaeology, University of London. During that time, they "interacted with many of the people cited" in this chapter, including Tixier, Madsen, and Texier (Bergman, July 27, 2009, personal communication). In their experiments, they followed up on observations Newcomer (1975) made about hard- versus soft-hammer blade production in the European tradition (see Barnes and Cheynier 1935; Barnes and Kidder 1936; Bordes 1947, 1948; Knowles 1953). Ohnuma, Bergman, and Newcomer each made blades and flakes of fine-grained flint with hard and soft percussors and then selected a sample of flakes from each experiment for identification of flaking modes (hard- or soft-hammer percussion) by the other members of the team in a blind test.

Flakes were analyzed individually according to the attributes of their bulbar areas and identified as per knapping mode (see Newcomer 1975). They derived a series of criteria from their blind tests for reliably distinguishing flakes made by hard-hammer percussion versus those made with soft stones or antler percussors. After this early experiment, Ohnuma and Bergman pursued different research interests, with Bergman focusing on analysis and projectile technology (e.g., Bergman 1987, 1993; Bergman and McEwen 1997; Bergman et al. 1988, 1990; Bergman and Newcomer 1983; McEwen et al. 1991; Miller et al. 1986). They met up again for experiments in making microblades.

Ohnuma's microblade experiments followed the same protocol as the flake experiments. These later experiments began in 1991 while Bergman was a research fellow in Japan. Ohnuma and Bergman "had learned some tricks from Madsen while in London, and [they] created bifaces like Kobayashi's System A … [using] a device for immobilizing cores … basically a fork set-up with a notched basal platform" (Bergman, July 27, 2009, personal communication; Ohnuma 1993: 175, note 2). Following the more recent French tradition from Tixier (1984b) to Pelegrin (1984b, 1991, 2003), Ohnuma and Bergman attempted to identify the technical stigmata for blades made by direct percussion, indirect percussion, and pressure. Ohnuma reduced obsidian microcores with these three techniques. He then selected 100 microblades from each experimental series for analysis of metric and nonmetric attributes. Force was applied to some microcores with chest and shoulder pressure; other microcores were held in a natural "graspable vise" (a section of forked tree limb, similar to Sollberger's forked branch [Fig. 3.5a]), and this was held in the hand for a freehand pressure technique (Ohnuma 1993: 162). Another triad of similar experiments was performed by Masaju Kubota, and 50 microblades were selected from each of his experimental series and compared with those from Ohnuma's experiments. Other experiments with these techniques worked cores of siliceous shale. From these experimental blades, diagnostic attributes and metric ratios for distinguishing microblades from the different manufacturing techniques were determined, and these were then used to analyze microcore industries from Iraq and Japan.

A few observations from the study are of particular interest to the general question of pressure blade technologies. Ohnuma's (1993: 172) reaction to the artifacts from two Middle Eastern sites reveals his practitioner perspective:

> The micro-blades from these two sites were so regularly-made that the present author was convinced at first sight that they had been detached by pressure technique; they typically bore such characteristic features as Tixier had proposed for pressure flaked débitage and cores (1984b: 66), i.e. micro-blades with regular thinness/flatness, parallel/straight dorsal ridges/edges, smooth ventral surfaces, narrow butts and short pronounced bulbs, and cores with regular flake scars and pronounced negative bulbs left by uniform and thin/flat micro-blades removed.

This is a common gestalt reaction of flintknappers to collections and a source of irritation between knappers and analysts who don't break rocks. It anticipates Pelegrin's (2003: 57) comment, "one can only recognize what one already knows." The bridge between the two is to make explicit the tacit knowledge that comes from making and handling stone tools of various types. Ohnuma made some of these features explicit. Some attributes were useful for distinguishing pressure blades from percussion blades, others for distinguishing between direct and indirect percussion blades. As with the analysis by Sollberger and Patterson, these distinctions hold for populations of blades compared to each rather than for individual specimens.

3.2.11 Dan Healan and Janet Kerley: Core Immobilization and "Counterflaking"

Getting back to Mesoamerica, Healan and Kerley (1984) published an article based on a knapping experience Healan had at Flenniken's flintknapping fieldschool while learning the technique for reducing cylindrical cores with the use of a Crabtree vise. Healan observed that four blades came off short from his preformed core and exhibited a peculiar kind of edge attenuation and damage. After each failure, Healan placed the stunted blade back on the core to determine what had happened, and he found that the margins of damaged blades had come into contact with the vise. "Areas of contact are always the most salient parts of the body …" (Healan and Kerley 1984: 3). When removing blades from the corners of the core, the future edges of some planned blades contacted the wood of the vise during removal and splintered in a particular way, a pattern Healan christened "counterflaking." This same year, Pelegrin (1984c: 115) independently published the same damage pattern, which he called "inverse retouch" (*retouche inverse*). This has since been presented in English by the awkward term, "reverse scratching" (Pelegrin 2006: 53). The illustration accompanying this last description verifies that it is the same phenomenon described by Healan and Kerley. Reverse or inverse retouch occurred on blades made from cores immobilized in a three-point holding device (see below).

Healan isolated four common features of counterflaked blades, and he and Kerley identified this damage pattern on ten blades found in blade workshop debris from the Postclassic site of Tula, Hidalgo (Fig. 3.9). They correlated the archaeological with the experimental and proposed that the damage to the Tula blades derived from the same cause as the experimental ones and thus were evidence of the use of wooden vises at the Tula blade workshops. As implicated by Pelegrin's observations, this hypothesis was too restrictive.

I became aware of Healan and Kerley's study before it was published and was excited by the possibility of a clear marker for identifying vises, something the Crabtree circle thought about. On July 30, 1983, I spent seven hours trying to duplicate counterflaking. I made cores for pressure reduction in a vise and purposely removed blades in the contact zone where an edge of an intended blade would come off the part of the core in contact with wood. I broke many blades and ruined three cores. Some of these blades exploded coming off their cores. What surprised me more, however, was that some did not. For some, removal of the blade forced the jaws of the vise open for a microsecond and allowed the blade to fly free without damage. After spending a morning traumatizing blades, and not producing any counterflaked ones, it occurred to me to check blades made years earlier with foot-held cores.

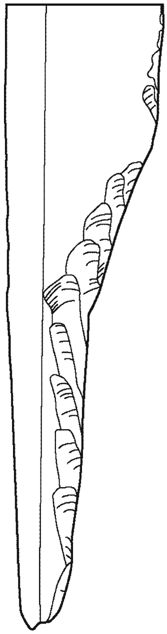

Fig. 3.9 Tip of a counterflaked blade (Drawn from a photograph in Healan and Kerley 1984: 5, Fig. 3e)

I found three counterflaked blades in these experimental collections. I concluded from this that counterflaking is not unique to vise-made blades (Clark 1984). I later found Pelegrin's reference to counterflaked blades associated with fork-held cores, so counterflaking is a general phenomenon of blademaking.

3.2.12 Errett Callahan: Flint Danish Microcores

A compelling case can be made that Callahan has done more to promote experimental archaeology and international dialog about stone tool technology than anyone since Crabtree (see Harwood 1986). Callahan is a master knapper and flint artist who is largely self-taught in the basics, beginning in the late 1950s. Going back to early grade school, Callahan was fascinated with the outdoors and woodlore; he made his first bow and arrow at the age of five and his first (slate) arrowheads in the seventh grade. But he really got started in 1956 while working a summer at the general store at Old Faithful in Yellowstone National Park – chipping out arrowheads from obsidian and glass in his spare time (Callahan and Titmus 1999; Harwood 1986: 1). After about 10 years of self-teaching, he became aware of other knappers.

> I will never forget that day in 1966 when, after plodding along alone for ten years and thinking that I had single-handedly rediscovered the "lost" art of flintknapping, I stumbled upon F. Clark Howell's book, *Early Man* …. In those pages I was confronted with an amazing

> French wizard with such spectacular knapping abilities that my little "arrowheads" suddenly seemed like embarrassing moments of play. I was both deflated and elated. A master far greater than I could ever be; a model for years to come.
>
> (Callahan 1981: 2)

After becoming familiar with Bordes's work, Callahan was profoundly influenced by the writings and work of Crabtree, Bruce Bradley, Sollberger, Titmus, Pelegrin, Madsen, and Richard Warren (Callahan 1995c; Harwood 1986: 2; Watts 1997). As did other American knappers considered here, Callahan started by making arrowheads and bifaces and then moved on to other technologies. Following his magnum opus on Clovis technology (Callahan 1979d, 2000c; see photo-essay in Kopper 1986: 42–43), he progressed to Danish flint technologies, including the production of microblades.

Callahan's (1984, 1985, 2000a) two experimental studies of flint microcores from Denmark were meant to resolve archaeological questions, as well as demonstrate best practices of how replication studies ought to be performed (1995c). He appears to have conducted these experiments in the reverse order of their publication, so I consider them in real time to maintain the experimental progression. His study of Mesolithic Danish microcores from Vedbæk, intended as a tribute to Crabtree in 1982 (1985: 38; also, Pelegrin 1984c: 110), addressed three hypotheses: "(1) Microblades were removed from cores which were secured in a holding device. (2) Microblades were removed from their respective cores by means of a fabricator of an antler-like material. (3) Microblades were removed by hand-held fabricators" (Callahan 1985: 27). To test these ideas, Callahan reduced eight cores. Of these, five were by pressure. He started with his experience with Crabtree's technique and went on from there. By this time, Callahan had his company, Aztecnics, which sells obsidian pressure blades (see Harwood 1986: 2), so he had made "thousands of blades" (Callahan 1995a: 225) by this time. He preferred "a gentle building up of force so that the moment of release comes as a surprise" (Callahan 1995b: 84) – rather than by a "lunging pressure." Experiment 4 combined a copper-tipped tool with a handheld clamp for the core. This was followed by an experiment with the same tool, with the core being held in the hand. Experiment 6 was a handheld core combined with an antler tool. This turned out to be the most difficult exercise and raised issues addressed in two additional experiments.

Callahan's hand clamp was derived from a device first used for fluting Folsom points. This clamp affected how the microcores were designed and prepared.

> The clamp which was used for these experiments [Figure 10] was inspired by a clamp which I witnessed J. B. Sollberger employing for Folsom fluting … (I had previously used a clamp which gripped with two contact points instead of three.) For such a clamp … it was necessary for the core to be quite parallel-sided for a secure seating in the clamp. Otherwise the core would tend to rock back or forth during the stress of pressing …. The cores which were hand-held did not have to be quite so parallel-sided, as the hand could accommodate some degree of irregularity here with no repercussions ….
>
> (Callahan 1985: 34, 36)

In his fourth experiment, 80 microblades were produced with the use of a copper-tipped tool from a clamp-held core. The core in Experiment 5 was handheld,

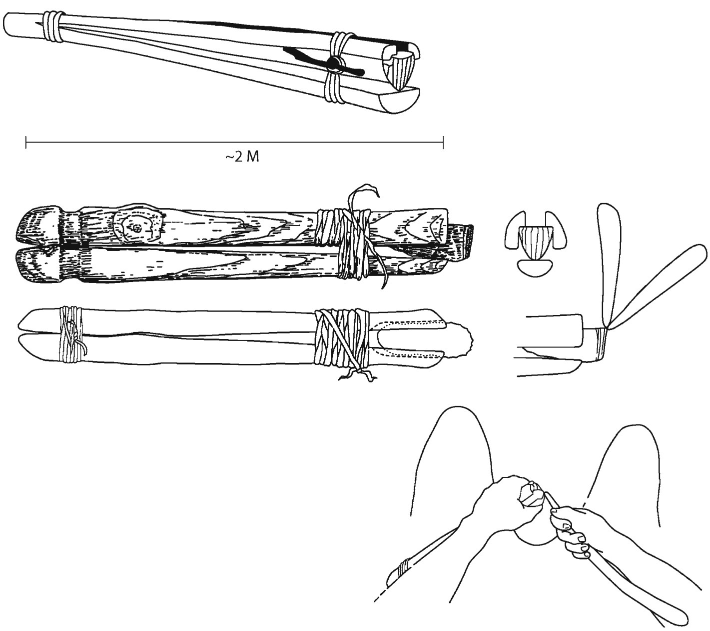

Fig. 3.10 Split vertical vise used by Errett Callahan (Redrawn from Callahan 1985: 33, Fig. 5f–m; see also, Pelegrin 1984c: 110, Fig. 5; Tabarev 1997: 145, Fig. 2)

and 40 microblades were made. "The smallest core which I could grip by hand tight enough to allow the removal of blades was about 5 cm in length" (1985: 34). This parameter relates to hand size, and Callahan claims to have large hands (ibid.).

Experiment 6 was the pivotal one of the series. Callahan reduced the core involved with a short antler pressure flaker while hand-holding the core.

> Despite the use of a shallow core (of minimum depth), it was exceedingly difficult to remove any microblades. A longer antler tine was used for a while, being held in the same manner as the shorter one, but the results were equally poor. In actual fact, 65 microblades were removed; but as detachment required all the strength I could muster, with the antler tip slipping off the platform repeatedly … and as the blades being questionably within the acceptable range of variation, and did not approach the length of the average artifact blade … My inability to remove sizeable microblades surprised me because of the success with which I had removed relatively long blades with antler in the past with much smaller cores …. These cores, however, while being flaked with the same short antler tine I was now using, had been secured in a clamp ….
>
> (Callahan 1985: 31)

Experiment 7 reconfirmed his previous experience. This core was secured in a clamp, and a long antler tine was used as the flaking tool. Callahan (1985: 31) observed "that a primary advantage of the clamp was to increase leverage by having the far end of the clamp resting along the left forearm, thus preventing hand/core movement" (Fig. 3.10). Sixty microblades were made from this core with relative ease, and they ran the full length of the core. Experiment 8, the last, revisited the frustrations of Experiment 6 with the insights gained from Experiment 7. The core was handheld, but a long antler tine was used to remove blades. Thirty blades "were easily removed from the core" (1985: 31).

On the basis of these experiments, and with observations of artifacts in mind, Callahan concluded that "There is a high degree of probability that … blade removal was made on cores which were hand-held and was effected by pressure with a hand-held fabricator of antler or an antler-like material" (1985: 37, emphasis removed). He made blades with other tools and with the aid of clamps, so some of the deciding logic was based on the "principle of least complexity" (1985: 37) – favoring the simplest way blades could have been produced.

Callahan's second project involved an unusual Danish core with an obtuse platform. Before planning his experiment, he called upon the experience of fellow knappers. He received commentary from eight knappers to help him understand a microcore with platform angles of about 113°, well in excess of the 90° limit mentioned by Sollberger and Patterson (above; Patterson 1981; Sollberger 1986a). It turns out that this unusual Danish core is not unique in its archaeological context. How was such a core produced? Consensus opinion was that it was worked by pressure with an antler tool, with the core immobilized in some sort of holding device, and with an emphasis on outward rather than downward pressure (Callahan 1984: 90). In conjunction with this project, Callahan (1984: 88–89) summarized an important experiment by Anders Fischer (1974) not otherwise reported:

> The best replicas seem to have been made by Fisher. Of all respondents, he, as a Danish archaeologist, has had the most experience in both handling and replicating Maglemose cores. He says, 'Some years ago I did a series of simple experiments … I found out that nice microblades could easily be made using pressure sticks of red deer antler tine. For the sake of ease, the cores were fixed in a carpenter's bench. The pressure stick was then positioned close to the platform edge, and when quickly pressed downwards and outwards … regular microblades could be peeled off one after the other – only delayed by the necessary trimming of the platform edge (Fischer 1974: 164). In this process, the platform angle gradually increased from the starting point of around 70 [degrees] to more than 100 [degrees]. The practical limit of flaking was reached at around 110 [degrees] … *These platform angles do not prohibit controlled flaking patterns*' [Callahan's emphasis].

With these guidelines, Callahan prepared an obsidian core with a sawn platform which he reduced in a vise using a copper-tipped chest crutch (1984: 91). The purpose of this experiment was to determine whether he could produce blades from an obtuse-angled platform rather than to replicate the core under consideration. Once he had preformed a core, he removed a series of blades:

> The downward-outward force relationships were manipulated so that outward force predominated. Thus, downward chest pressure was applied only to the degree which would allow blade removal without having the tool slip from the core. Then, with *no* increase in downward pressure, outward pressure was applied until the blade released. I would estimate that outward pressure was two or three times that of downward pressure. This amount of outward pressure tended to insure blades which terminated at, or shy of, the distal end of the core and to inhibit the tendency for overshooting. Such blades are quite straight and had only the slightest degree of distal curvature.
>
> (Callahan 1984: 91)

This procedure resulted in an exhausted core that was a close match to the archaeological specimen. Particularly interesting was the manipulation of forces in this experiment to resist running blades to the very end of the core. Callahan argued that a different pressure stroke would have given his core a different morphology.

Callahan made 85 blades from his core, and these represented 14 different series of sequential removals. The angle of the platform to the core face increased with each series of blades removed. He terminated his experiment when the angle of his core was 112°, even though another series of blades could have been removed (1984: 92). The increasing platform angle did not adversely affect his removal of blades. Callahan had to take care not to overshoot the distal end of the core. Plunging blades tended to keep his platform-to-core face more stable (1984: 92, 94). Thus, deliberate blade overshots would have been a way to maintain an acute-angled platform and to keep the core in production longer.

3.2.13 Jacques Pelegrin: Handling and Working Cores of All Shapes and Sizes

Following Bordes's and Tixier's efforts, Pelegrin has amplified the venerable French traditions of making flint and obsidian blades, emphasizing different techniques for immobilizing and reducing cores of different sizes and shapes (see Pelegrin 1984a, c, 1988, 2003, 2006); he has also relied on his knapping experience to advance theory (1985, 1986, 1990, 1993, 2005). Pelegrin is largely a self-taught knapper who has had significant interaction with the ascending generation of stoneworkers. He began knapping at the age of 12 after seeing the Bordes's photo-essay in the *Early Man* book. At the age of 16, he met Tixier and saw him work briefly but did not receive instruction from him. His most formative influence came from Bordes with whom he worked for six summers, 3 weeks each summer, beginning at 16. Pelegrin made contact with Crabtree just before he died but never saw him knap (Callahan 1982: 63). Pelegrin's early work was with direct percussion techniques, with a later move to pressure techniques and various means of making blades. He started making pressure blades in 1980/1981 (Pelegrin 1984b: 118; Callahan 1982, 1984, 1985: 36; Tixier 1984b: 58). Since this time, Pelegrin has covered much of the same ground charted in Crabtree's (1968) blade study, but in a much more methodical way. Pelegrin has experimented with various kinds of flint and obsidian and made blades from cores of different sizes by direct and indirect percussion (1991, 2003, 2006; Pelegrin et al. 1991; also, Callahan 1982) and by direct pressure and lever pressure (1984a–c, 1988, 2003, 2006). He has also experimented with various holding devices and tools, including flexible and inflexible gut crutches with antler bits. He found crutches with flexible shafts to be superior to rigid crutches for making flint pressure blades, and he suggested that a flexible tool may have been used to make the longest Aztec blades (Pelegrin 1984b: 123, 2003: 59).

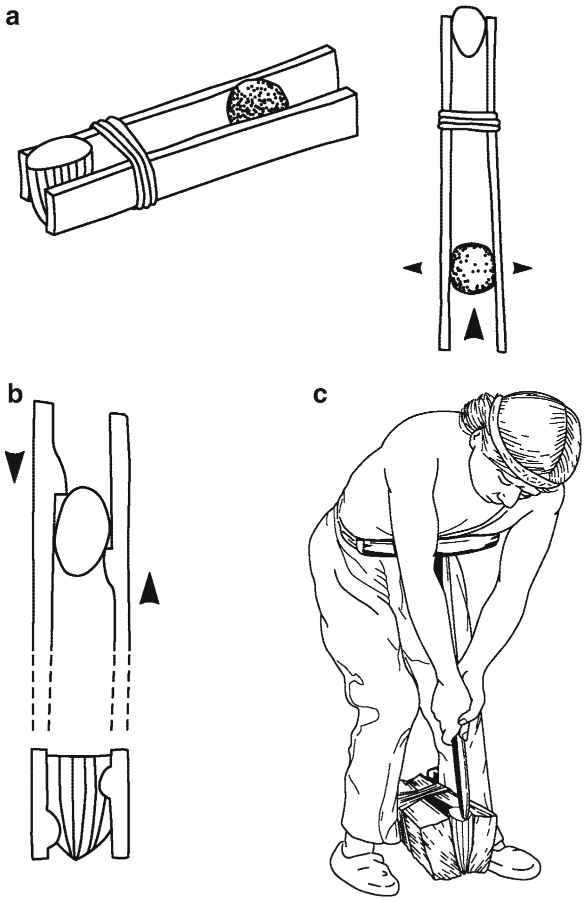

Fig. 3.11 Vises with lateral jaws. (**a**) Side and top views of a Crabtree clamp. (**b**) Variant of a Crabtree clamp with backstop and forestop ((**a**) and (**b**) redrawn from Pelegrin 1984c: 107, Fig. 2). (**c**) Reconstruction of use of lateral clamp (Redrawn from Crabtree and Gould 1970: 187, Fig. 7)

Pelegrin's experimentation with different working positions and ways of immobilizing blade cores is particularly innovative. He investigated differences between Crabtree's technique and my own and experimented with holding cores with clamps, vises, and gravity. Because his guiding interest at the time was flint blades, which by all accounts require more force to make than obsidian blades (1984b: 118), he did not experiment with foot-holding techniques (1984c: 105). Rather, he considered the following variants of vises with lateral jaws, clamps with vertical jaws, and three-point devices for stabilizing cores:

1. The best known option is a wooden vise with flat, lateral jaws (Crabtree 1967: Fig. 2; Sellers 1886: c874) in which two flat boards are tied together, the core inserted at the short end, and the boards spread at the opposite end to secure the core (Fig. 3.11a).
2. Pelegrin (1984c: c107, Fig. 2, translated by Charlotte Laporcherie) proposed a modification of the Crabtree clamp to make it more efficient. He suggested the

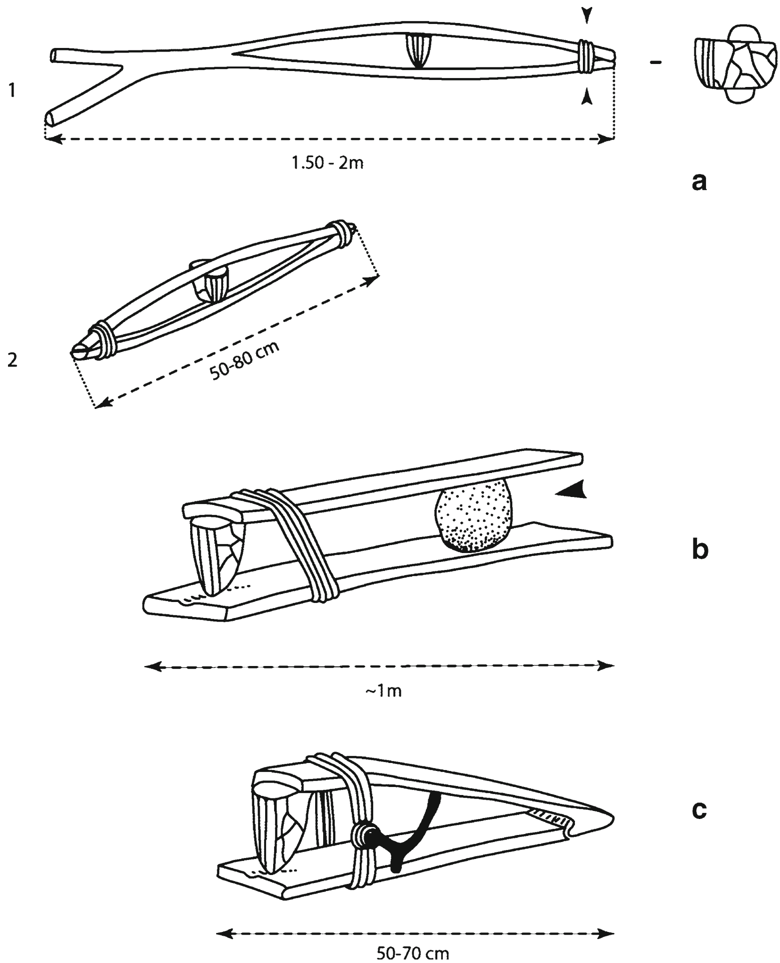

Fig. 3.12 Clamps with vertical jaws. (**a**) Horizontal clamp used by Bo Madsen (Redrawn from Pelegrin 1984c: 108, Fig. 3). (**b**) Vertical clamp that is a Crabtree vise turned on its side (cf. Fig. 10a). (**c**) Vertical clamp with back hinge and tightening system ((**b**) and (**c**) redrawn from Pelegrin 1984c: 109, Fig. 4)

addition of two supports to minimize movement, "one forward and at the top of one of the planks, the other at the bottom and more at the back of the second plank" (Fig. 3.11b).

3. Other options for stabilizing cores are to clamp them vertically rather than horizontally. Figure 3.12a illustrates a device used by Madsen. This is a Y-shaped branch, with the two short ends of the forked branch providing stability for the device when placed on the ground, and with the third member split horizontally so a core can be inserted. The core is placed transversely in the split branch and squeezed by tying the two half branches together (cf. Fig. 3.10).

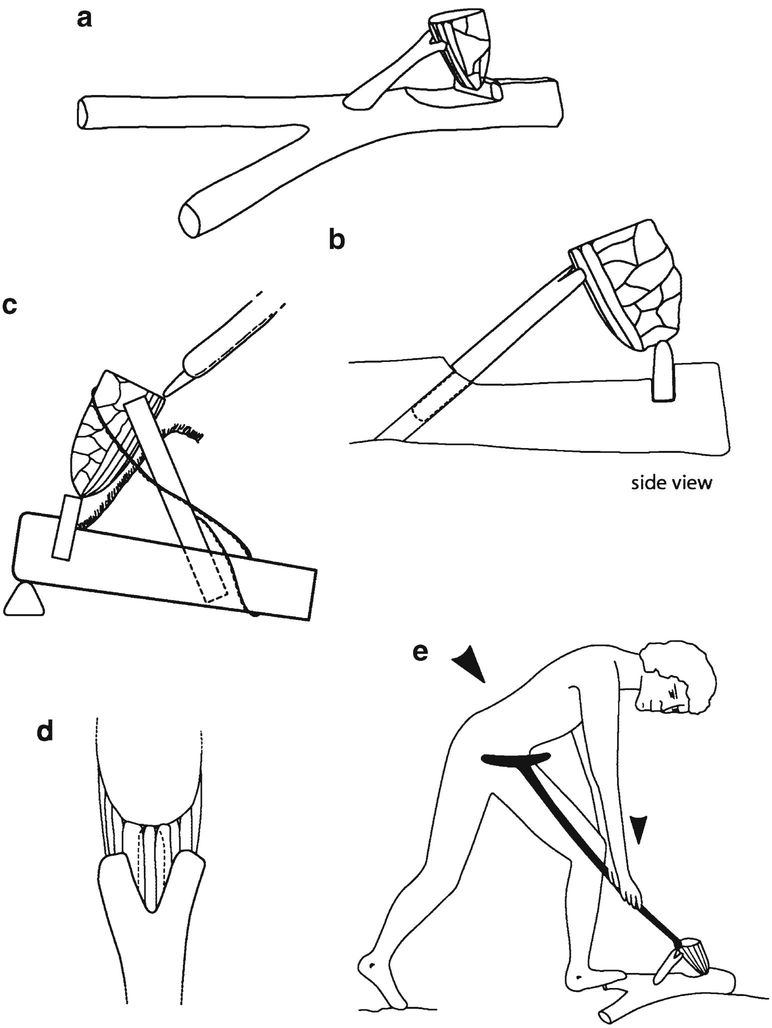

Fig. 3.13 Forked stick device for immobilizing cores. (**a–d**) Positions of the slot and forked stick (Redrawn from Pelegrin 1984c: 111, Fig. 6; see also, Piel-Desruisseaux 1990: 45, Fig. 35). (**e**) Use of devise with a gut crutch (Redrawn from Pelegrin 1984b: 120, Fig. 3)

4. Two versions of a vertical clamp using boards are shown in Fig. 3.12b, c. The first is essentially the Crabtree clamp turned on its side; the second has a hinge at the back and a means of tightening cords at the front of the vise to press on the platform and distal end of the core inserted vertically in the clamp. These are simplified versions of Sollberger's vise (see Fig. 3.5b).
5. Pelegrin's (1984c: 111, Fig. 6) forked device is a modification and extension of the fork apparatus described by Sollberger and Patterson (Fig. 3.5a). Pelegrin's fork requires three points of contact (Fig. 3.13), as with Callahan's device – a distal rest and two points just below the platform. It is a self-tightening device

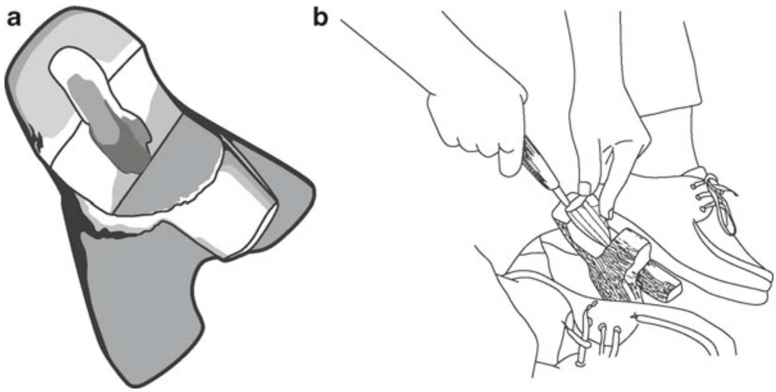

Fig. 3.14 Pelegrin's self-immobilizing core device. (**a**) slotted stick with a stabilizing foot (Drawn from photograph in Inizan et al. 1999: 149, Fig. 73.4; see also, Pelegrin 2003: 62, Fig. 4.8). (**b**) Manner of using the slotted stick (Redrawn from Inizan et al. 1999: 31, Fig. 4.5)

that takes advantage of the forces exerted in working, rather than fighting against them. As illustrated in Fig. 3.13a, this appliance takes advantage of two triangulations. The base support is a Y-shaped branch to be placed on the ground to provide stability (see photo in Inizan et al. 1999: 149, Fig. 73.2). The other points of vertical contact are supplied by a smaller forked branch inserted into the larger one, with a notched place in the main branch to lock in the distal end of the core (Fig. 3.13c). There are two stabilization challenges in making blades; one is to secure a core in a device, and the other is to secure the device itself (cf. Sørensen 2006: 285, Fig. 5).

6. A vertical triangulation setup can be made from a single piece of wood that can provide the horizontal stability and a vertical dimension. This is done by replacing the forked stick with a slotted stick (Fig. 3.14). The two edges of the slotted piece of wood serve the same function as the vertical forked stick (see Pelegrin 2003: 62, Fig. 4.8; also, Inizan et al. 1992b: 96, Fig. 46). Demonstrations of this technique can be seen in two films by François Briois (1992, 1995).
7. The same principles of stabilization through gravity can be built into a permanent system by making the appropriate modifications to a tree root (see Sellers 1886: Fig. 6). As Pelegrin (2003: 58, Fig. 4.2) described it, "one could scoop out a notch on the side of a root at ground surface and set the distal end of the core on a block of wood at the bottom of a hole dug in front of the notch" (Fig. 3.15a).
8. An even simpler system is to "scoop out the center" of a billet-like piece of wood and stand it up "obliquely against a large stone" (2003: 58, Figs. 4.3, 4.4). As shown in Fig. 3.15b, the hollow billet has a pointed end that would be stable – under vertical pressure – in a slight notch in the ground. All of the forces in this stable device are vertical, with the holding device stabilized in the same way and with the selfsame forces that keep the core stable in the slotted device.
9. Slotted devices can be made of different calibers for cores of different sizes. The distance between the opposed edges of the open slot (or the two prongs of

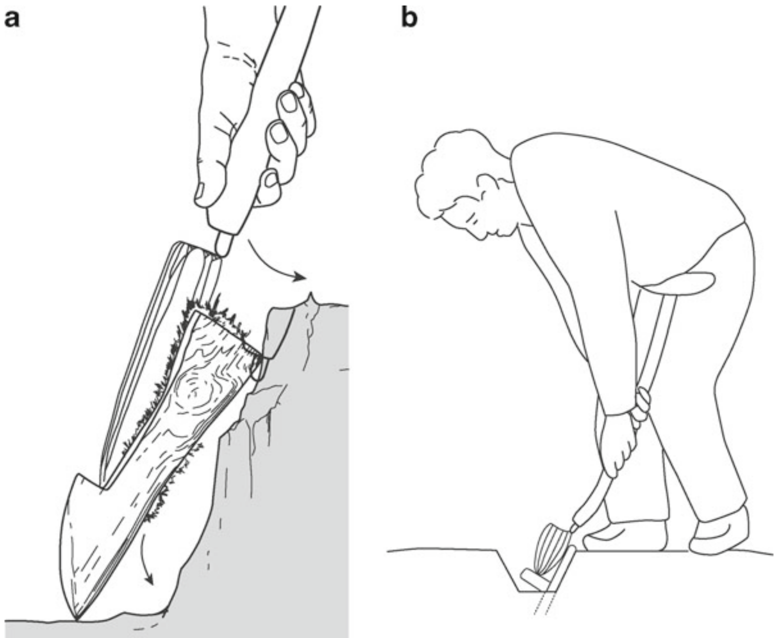

Fig. 3.15 Another of Pelegrin's self-immobilizing devices. (**a**) Using a hole to help secure a core (From Inizan et al. 1992a: 666, Fig. 2). (**b**) Slotted stick holding device without a horizontal foot (Redrawn from Pelegrin 2003: 59, Fig. 4.4)

a forked stick) determines the maximum width of the blades that can be safely removed without damage. Small versions of this device can be held in the hand to remove blades from microcores (Pelegrin 1988: 43, Figs. 2, 3). They can be made of wood or large mammal bone; the "end of a long bone with its medullary cavity can easily be used as a holding device" (Pelegrin 2003: 61, Fig. 4.7; for illustration of experimental microcores, see Inizan et al. 1992a: 664, Fig. 1).

10. Pelegrin employs the same principles for securing cores over 30 cm long as he does for those under 6 cm: a notched rest for the distal end of the core and two points of contact for opposite sides of the proximal end of the core. For his lever pressure technique, he makes a slotted device in a large log of sufficient weight that it is stable under the forces exerted. Because direct pressure is not used, Pelegrin also needs a fulcrum point for the end of his lever tool, as shown in Fig. 3.16.

A crucial insight of Pelegrin's studies is the complementarity between core shapes and means for keeping cores still. Each immobilization device anticipates an optimal core form and shape. In turn, ways of keeping cores still relate to the tools used to make blades of different sizes and the working positions involved. In a rational and efficient world, the size and shape of core preforms would correspond to production techniques and the manners of paralyzing cores for blade production. Morphological transformations of cores during reduction are critical clues for identifying the tools and techniques of ancient blade manufacture. Different techniques

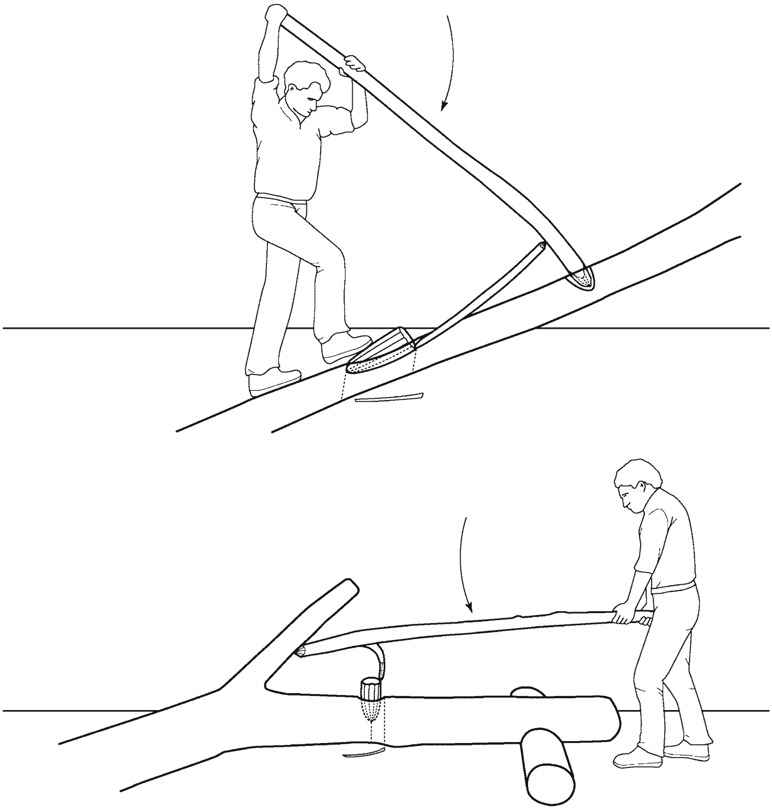

Fig. 3.16 Drawings of slotted logs used with a lever for removing very long blades (Drawn from photographs in Pelegrin 2003: 65, Fig. 4.11)

and core forms also dictate ideal sequences of blade removals, something that can be learned from studying expended cores and from refitting whole blades into their sequence (see Astruc et al. 2007; Kelterborn 2008c; Texier 1984b). The transverse arc of a core face also determines the blade width-to-thickness ratio, the likely number of dorsal ridges, and blade cross section.

In a recent appraisal of Mesoamerican blade experiments, Pelegrin addressed four unresolved issues. These are the techniques needed for making blades from different kinds of cores (large, slender, or small) and the role of indirect percussion in the reduction sequence. Pelegrin (2003: 56) repositioned this last question. Earlier researchers such as Coutier (Cabrol and Coutier 1932) and Crabtree (1968) explored indirect percussion as an option for making fine blades. For Pelegrin, this is no longer a credible hypothesis given the regularity of fine blades and the potentials of indirect percussion (Inizan et al. 1992b: 23), but this does not take the technique completely out of the picture. Indirect percussion was probably used to preform cores in some regions of Mesoamerica (Titmus and Clark 2003: 91); the data for this claim are the punches themselves (Clark 1985, 1988). Pelegrin (1984b, 1988, 2003,

2006) relies on principles of biomechanics and geometry to argue for size limits for blades made from particular raw materials with specific techniques and tools. He has been able to make blades 27 cm long with direct pressure (blade width is more restrictive than length), and blades much longer than that with lever pressure (Pelegrin 2003: 60, 2006). Medium-sized blades can be made in many ways. Small blades or microblades, however, present interesting challenges.

3.2.14 Jeffrey Flenniken: Flint and Obsidian Microcores

Flenniken has replicated a wide range of artifacts (1978, 1981b, 1985; Flenniken and Raymond 1986; Flenniken and Wilke 1989), including microblades (Flenniken 1987; Flenniken and Hirth 2003). He is a self-taught knapper, but he did study with Crabtree in 1973 and learned from him (Flenniken 1981a). At Crabtree's recommendation, Flenniken assumed responsibility for the flintknapping fieldschool when Crabtree retired, a circumstance that led to years of collaboration and interaction with him (Flenniken, in Callahan 1978b: 16, 21). Flenniken learned blademaking from Crabtree and has made hundreds of fine obsidian blades (see Lampe 1991: 659).

Flenniken's (1987) first study of microcores concerned artifacts from eight Paleolithic sites in Siberia related to the Dyuktai blade technique. After studying the cores, blades, and related debitage from these sites, he performed a series of experiments with flint of similar quality and configuration. The Dyuktai technology is a Type A system similar to a Japanese technique demonstrated by Crabtree (1972e) in *The Hunter's Edge*. A thick biface is made, one margin of the biface is removed with a "ski spall" flake to create a platform, and then blades are removed from what is essentially a long narrow core, ideal for holding in a vise (cf. Kelterborn 2003: 127, Fig. 8.9; Texier 1984b: 26, Fig. 2). Much of Flenniken's argument is carried by his drawing of the reduction sequence and his photographs of replicated artifacts rather than his descriptions. He reduced 25 bifacial cores and averaged about 101 "useable" (i.e., trapezoidal cross section) blades per core (Flenniken 1987: 122). Flenniken argued that the Dyuktai blades and cores "could not be produced by any method (i.e., freehand pressure, freehand percussion, hand-vise pressure, indirect percussion, etc.) other than by holding the core tightly in a simple wooden vise …" (1987: 122). He does not mention, however, any experiments with other techniques, so his confident identification is not supported by argument. He does propose that the blades and cores were made by pressure, and he relied on a list of technical stigmata similar to that proposed by Tixier. Flenniken identified seven attributes:

> (1) blades are small and uniform, (2) blade margins are parallel, (3) all blade arrises (dorsal surface) are parallel, (4) blades are thin and evenly proportioned, (5) blade platforms are small, exhibit minimal platform contact, or show preparation in the form of abrasion, (6) ventral blade surfaces exhibit few, if any, compression rings, [and have] diffuse bulbs of force, and feather terminations, and (7) blade scars present on exhausted cores also exhibit the above attributes …. It is *impossible* to consistently produce percussion blades possessing the above attributes.
>
> (Flenniken 1987: 122)

Of particular interest is an argument Flenniken made for the use of vises. He noted that the exhausted cores are larger than one would expect had they been hand-held and that "usable" stone had been discarded. "The fact that 'usable' stone was discarded as 'exhausted' suggests that the pressure blade cores were held, during blade production, in a wooden or bone vise of some type. The cores were exhausted when they could no longer be held securely in the vise" (1987: 122). Flenniken came to the opposite conclusion for obsidian microcores from the Epiclassic (ca. A.D. 600) site of Xochicalco in highland Mexico.

Flenniken's collaborative project with Ken Hirth and Brad Andrews (Hirth et al. 2000, 2003) with Type B obsidian microblades was inspired by unusual features of Xochicalco obsidian cores. Exhausted blade cores from this site are tiny by any standard and likely indicate a different technique of blademaking than described for the Mexica who lived in the same area five centuries later. Flenniken, Hirth, and Andrews's reconstruction of the blade reduction sequence indicates that "exhausted" blade cores about 18 cm long were imported into Xochicalco and then were broken in half to make microblade cores (Flenniken and Hirth 2003: 99; Hirth 2003a; Hirth et al. 2000: 139, Figs. 7.1, 7.2, 2003). They estimate the size of starting blade cores at 8–9 cm long and 3–4.6 cm in diameter; expended cores were about 5 cm long and 1.1 cm in diameter (Flenniken and Hirth 2003: 99). Given prior history, cores came with very regular, straight ridges. The smallest exhausted cores are about the diameter of a pencil – 0.5–1.0 cm (Flenniken and Hirth 2003: 99; Hirth et al. 2003: 192, Fig. 13.10). How were these cores immobilized during blading? "The problem that the Xochicalco cores present for current technological reconstructions is that they are too small to have been held with the feet" (Flenniken and Hirth 2003: 99). "Experimentation with the foot-held technique by Gene Titmus has established that cores under 8–10 cm in length are difficult to hold with the feet unless the core exceeds 4 cm or more in diameter (see Titmus and Clark [2003])" (Flenniken and Hirth 2003: 99). The main idea is that techniques have inherent limits, and these can be bracketed experimentally.

Flenniken and Hirth explored the possibility that the microcores at Xochicalco were worked with freehand pressure. They pursued two related questions: core size and platform treatment. Most cores at Xochicalco have pecked and ground platforms. In his feasibility exercises, Flenniken made ten small, single-faceted cores by direct freehand percussion; the irregular ridges of these cores were removed by pressure. For each core, Flenniken held it in his left hand in a leather pad and used a deer-antler pressure flaker held in his right hand to make blades 5–7 cm long (Flenniken and Hirth 2003: Figs. 6.2–6.4). The "prismatic blades produced with the handheld technique were very similar to those recovered in the Xochicalco collections" (Flenniken and Hirth 2003: 100–101). The platforms of five other small cores were pecked and ground. Blades were pressed off cores of both types to assess the benefits of the special platform preparation (cf. Adams 2005).

Given the relative benefits of foot-holding and hand-holding techniques, Flenniken and Hirth (2003: 106) suggest that the two were complementary, with one technique picking up were the other left off: it "is precisely at the point where cores become too small and difficult to reduce using a foot-held technique (ca. 8 cm in length and 3–4 cm in diameter) that handheld prismatic pressure blade reduction became feasible." As discussed above, their proposal is not the only option for stabilizing and working small cores. Distinguishing among different ways of securing microcores must depend on features other than size.

3.2.15 Eugene Gryba: Chert Microblades

Gryba has been making microblades with handheld pressure since the 1970s (2006: 60, Figs. 5–7) but has only mentioned his experience in relation to fluting Folsom points (1988, 1989). He has made chert blades "in excess of 9.0 cm in length, and channel flakes as large as 2.2 cm wide, 2.3 mm thick, and 12.0 cm long that had an outre passé termination [sic]" (Gryba 2006: 62), and these with antler pressure flakers about 5–7 cm long. Gryba is 1.67 m tall and of slight build (1988: 57) so his success in producing long microblades and flutes in tough stone is not a consequence of his size. As he explains (1989: 66), "The relatively small size of my pressure flakers, and the method I adopted for holding and applying them … evolved from my experiments in microblade production."

Gryba's success derives from avoiding the Crabtree style of pressure blademaking, such as employed by Flenniken. I would characterize Gryba's technique as using a palm "crutch" (without a crosspiece) – pushing his flaker with the heel of his hand. His tools are not hafted, so he relies on thick padding for support (Gryba 2006: 61, Fig. 2). Mechanically, his method is akin to that described for Eskimo curved-handled pressure flakers: the application of pressure from the heel of the hand (see Holmes 1919: 319, Fig. 181). Watching Gryba flute Folsom points is a lesson in humility because it does not seem possible that someone of his lithe frame, with a tool that looks like an unusable slug of antler, can press off such long channel flakes and blades. It is possible because of his way of holding his tool and working.

3.2.16 P. V. Volkov and E. Iou Guiria: Long Blades with Lever Pressure

Volkov and Guiria (1991; Guiria 1997) reported a series of blade experiments performed in 1988 which produced more than 2,000 blades and explored the limits of blademaking with different techniques and materials (1991: 385). They illustrated one Type B flint blade from Petrovo-Svistounovo, Ukraine, that is 231 mm long, 26 mm wide, and 3.6 mm thick, having a length to thickness ratio of 1:64 (1991: 386). Their experiments show that such a blade cannot be produced by direct or indirect percussion or unassisted manual pressure. Rather, they calculate that a flint blade this wide and thick requires 300kg of pressure at the point of application (1991: 387). The material a blade is made of makes all the difference. They argued that raw flint blades require two-and-a-half to three times the force to remove as obsidian blades the same size (1991: 389).

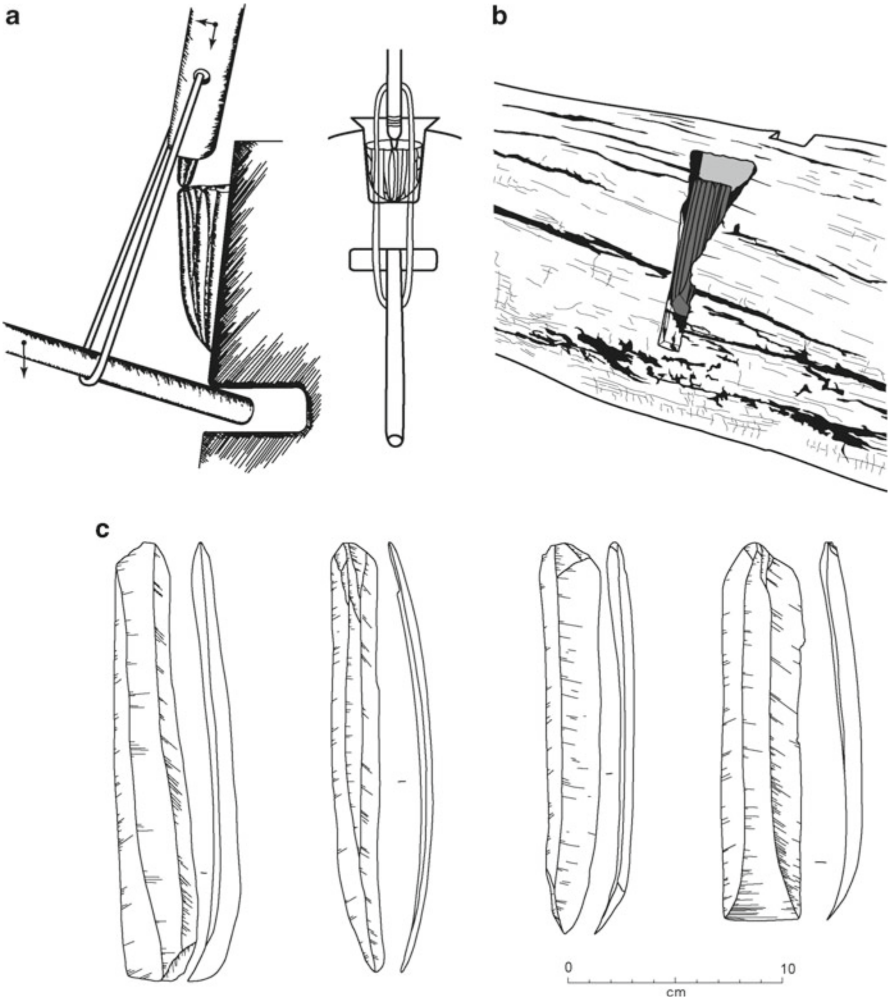

Fig. 3.17 Long blades and the means of making them. (**a**) Combination of a slotted holding device with lever pressure for making very long blades (Redrawn from Volkov and Guiria 1991: 386, Fig. 5). (**b**) Core shown in a slot in a cut tree (Drawn from photograph of Greg Nunn's device). (**c**) Long blades made with lever pressure (Redrawn from Volkov and Guiria 1991: 386, Figs. 7, 8)

One novelty of Volkov and Guiria's experiments is that they compared blades made with unassisted pressure to those produced with assisted, lever pressure. They do not provide details of their manual technique. I presume it was the standard one of Crabtree and Tixier, i.e., use of a copper-tipped chest crutch and a vise. Their version of lever pressure is illustrated in Figs. 3.17 and 3.18. They reported that the handle of their lever was 1.5 m long, but nothing else (1991: 388). As evident in their drawing and cartoons, this device is a modification of Sollberger's lever technique (cf. Fig. 3.5b), but with a rope instead of a wooden crosspiece connecting the

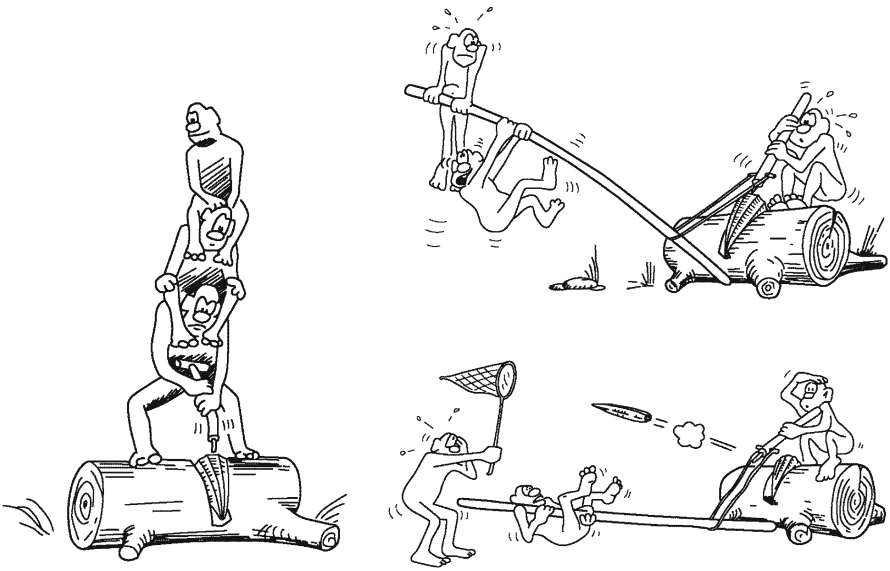

Fig. 3.18 Caricatures of the manufacture of long blades (Redrawn from Guiria 1997: 70, 73, Figs. 12, 17)

lever to the tip of the pressure flaker (see also Kelterborn's double-lever device, this volume). The means of holding the core is not so clear in the original drawing, but it is a self-tightening system. A large slot is cut in a large log or felled tree to hold a core; the slot is narrowest on the outside face of the log and expands or opens up toward the center of the log (Fig. 3.17b). Blades are removed from the exposed face of the core rather than from the back face of the core, as in Pelegrin's systems for long blade removal (Fig. 3.16). The clarifying illustration here is drawn from an experiment conducted by Greg Nunn (see below). Volkov and Guiria's lever technique is a two-person task. The cartoons reproduced in Fig. 3.18 come from Guiria's (1997) synthesis written in Russian. They show that the force exerted by lever pressure exceeds that which could be generated by direct pressure by three knappers in piggyback formation (something actually tried at Les Eyzies; Crabtree, August 27, 1979, personal communication). The cartoons depict the basic form and mechanics of the lever system. One worker slowly applies the pressure through the lever, and the other worker keeps the pressure flaker in correct position on the edge of the core until the blade is released.

Witold Migal (2006: 395) describes a modification of Guiria's and Pelegrin's systems for making long pressure blades found at Neolithic sites in Poland. He used a log for supporting the core:

> the core was made immobile in a carved notch. Blades were detached with an antler-tipped wooden pole put through a hole in the upper part of the log. The point of support for the lever had been constructed as a hole in the wall opposite the cores. This way of working was very compact and connected with the size and shape of cores. The longest blades were 21 cm, and I could exploit the core to about 15 cm of height.

Migal notes that simple machines, such as used to squeeze grapes, could also have been used to make blades (2006: 398, Fig. 8). Long blades and grape seeds appear in the archaeological record at the same time. A significant question addressed by Migal is why long blades had such a restricted modality in length and why some long cores were abandoned before all the possible blades had been removed. He hypothesizes that both phenomena relate to the requirements for immobilizing cores. "Precision in shaping of sides and backs of the cores leads us to the conclusion that a system of immobilization must have existed which excluded the core's further use after a reduction in dimension" (Migal 2006: 392).

A lever pressure technique with cores immobilized in tree trunks is capable of delivering almost unimaginable force to cores and can produce very long, wide, and thick pressure blades, as shown in Fig. 3.17c. Any limitations would be those built into the slots for holding cores and the size of the cores themselves. This system will accommodate much longer levers and significant multiplication of brute human force. As with the smaller, self-immobilizing devices, the same issues of vise stability are in play with a tree as with a small Y-shaped branch vise. "The tree needs to be well braced to eliminate the tree from rolling due to the force of the lever" (Nunn, July 22, 2009, personal communication; see Figs. 3.16, 3.18). Lever pressure is capable of putting more than 500 pounds of pressure on a core, and this is certainly sufficient for rolling even a very large tree trunk.

3.2.17 Philip Wilke and Leslie Quintero: Naviform and Microblade Cores

Wilke and Quintero's (1994; Quintero and Wilke 1995; Wilke 1996) experimental studies of blade cores and microcores provide numerous insights about how to immobilize small cores and of possible telltale traces left on cores and blades secured by different means. Wilke's first knapping experiences and archaeological studies were with projectile points and bifaces, and only later did he turn to blades. He started to knap on his own about 1970 but learned basic skills in a course taught by Jeanne Binning in 1984. He pursued his interest by attending Flenniken's flintknapping fieldschool in 1985, and he assisted in this fieldschool in 1988, 1989, and 1990 (August 3, 2009, personal communciation). At these fieldschools, he witnessed Flenniken make pressure blades with an enlarged version of his Dyuktai technique as well as the Crabtree technique, but it was not until he began teaching pressure blademaking in 1991 that he became proficient with these techniques. He attempted to use the Callahan system but ended up pressing a blade into his leg and gave it up. By 1998, he had also given up vise systems and had begun using jigs inspired by Pelegrin's systems (July 29, 2009, personal communication). For her part, Quintero attended the flintknapping fieldschool in 1990 and is an accomplished knapper and lithic analyst (P.J. Wilke, June 17, 2009, personal communication). Wilke and Flenniken cooperated on several replication studies of projectile points and bifaces (Flenniken and Wilke 1989; Wilke 2002), so it is fair to say that Wilke and Quintero both have been steeped in the Crabtree tradition through Flenniken, Titmus, and others; they are also acquainted with the principal French knappers pursuing blade studies in the Old World.

Quintero and Wilke's first study was of naviform cores and blades (Type A system) from Syria, Israel, and Jordan. Quintero brought some naviform cores to the 1990 fieldschool to solicit opinions on how they were made, and she and the other participants were able to observe attempts to duplicate naviform core blanks, mostly by Titmus. The issue of their production was not resolved, but it did become clear at the 1990 fieldschool that the naviform cores were for "percussion blades, not indirect, as the literature at the time claimed" (P.J. Wilke, July 29, 2009, personal communication). After returning from fieldschool, Quintero and Wilke experimented over the next several years and reduced 300–350 cores. Based on detailed studies of experimental outcomes, they concluded that naviform cores were reduced by direct percussion (Quintero 1998; Wilke and Quintero 1994: 40).

The reduction sequence for making naviform cores bears a formal and logical resemblance to Type A core technologies. The basic concept is to make a blade core by removing the edge of a thick biface or tabular piece of flint. An unusual feature of naviform cores is the manner of setting up platforms. The original biface is shaped like an isosceles triangle with one long side. In contrast to the Japanese and Siberian blade industries, the long edge of the biface or tabular piece of flint becomes the working face of the core from which blades are removed, rather than the platform. Naviform cores have two opposed platforms (the short sides of the triangle), and blades were removed from both. The bidirectionality of these cores served to keep the blades straight, and it also simplified the correction of some kinds of errors. These blades are not as regular or uniform as pressure blades, but they are regular (see Quintero and Wilke 1995: Figs. 1–4; Wilke and Quintero 1994: Plates IV and V). The uniformity of these blades, coupled with their small "punctiform" platforms, had fostered speculation that they must have been made by indirect percussion (Calley 1984, 1986; Cauvin and Coqueugniot 1988; Inizan 1980; Rollefson 1990; Suzuki and Akazawi 1971; Valla 1984). But Wilke and Quintero (1994: 40) have been able to reproduce all features of these blades by direct percussion with a soft hammerstone.

Wilke next studied more regular blades and bullet cores from the east Euphrates and Anatolia region. He went into his experiments of making pressure blades with considerable experience in making percussion blades. Wilke employed a version of Pelegrin's slotted block device, made from an elk metapodial, to extract blades from small cores (Type B), an idea going back to proposed techniques of fluting Folsom points by indirect percussion (see Tunnell 1977: Fig. 9; Warren 1968); see Fig. 3.19. Wilke (1996: 289) replicated small, bullet-shaped cores about 2.5–5.5 cm long and less than 1 cm in diameter. Bladelets from such cores generally qualify as microblades (less than 1.2 cm wide in his scheme). Wilke's experiments were designed to "determine the technological parameters" of bullet-shaped cores from the Near East (1996: 289). He drew attention to the distal shapes of cores and how cores were held, an attribute stressed by Barnes (1947b) in his comparative study of blade cores. Wilke came to the same conclusion as Barnes that "Pointed and Chisel-ended

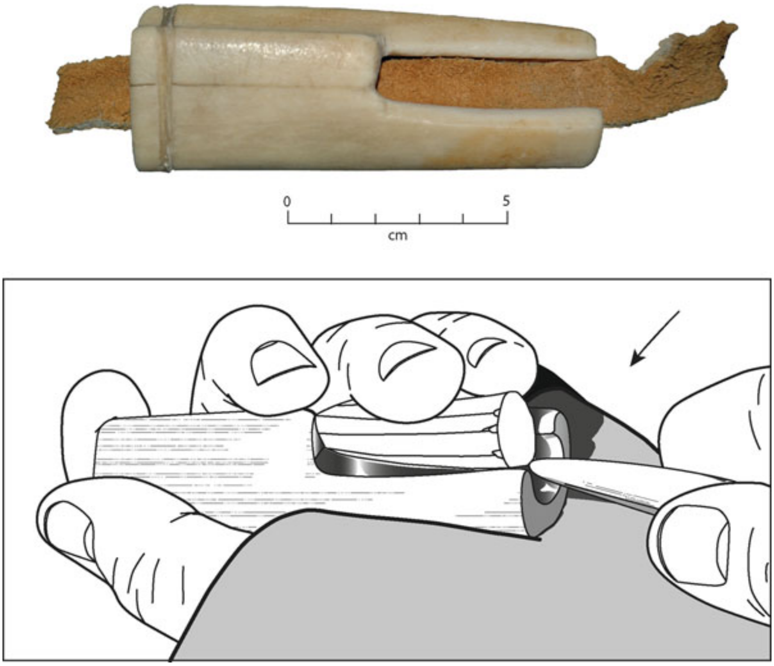

Fig. 3.19 Slotted bone device and its leather strip (to prevent blade breakage) for hand-holding microcores (Photograph courtesy of Phil Wilke, and drawing from a photograph in Wilke 1996: 295, Plate 1)

types of core do not seem suited for use in the palm of the hand. It is probable, therefore, that an anvil was employed and that there were local variations in its character" (Barnes 1947b: 109). In Wilke's technique, the anvil feature was built into his holding device:

> holding the cores in the hand with no distal support … becomes more difficult as core diameter diminishes … experience indicates the technique cannot encourage the consistent production of straight blades … distal core support is essential for detaching straight blades, which in turn maintain the straightness, and the length, of the core face. Distal support causes the detaching force to compress the part of the core where the blade is to be detached, thus directing the fracture to the point of support. Short, straight blades can be produced more consistently using simple but effective hand-held appliances of wood or bone that provide necessary lateral and distal core support, as well as complete core immobilization ….
>
> (Wilke 2007: 222)

Given the regularity of blade margins, ridges, and curvatures, Wilke argued that blades from Near Eastern microcores were made by pressure rather than by direct or indirect percussion (Wilke 1996: 290, footnote 3).

Through trial and error, Wilke identified five critical parameters and/or desiderata of microblade production: (1) "Cores must be immobilized to prevent movement and consequent change in direction of loaded force during blade detachment" (1996:

293). (2) “Cores must be supported at their distal ends, both to direct the detaching force to the end of the core and to stop the detaching fracture at that point. Thus, support encourages the production of straighter blades that run the full length of the core without significant overshot” (1996: 294). (3) “Cores must be supported distally in a manner that leaves room for blades to detach freely from the distal end of the core” (1996: 294). (4) “Cores must be supported in a manner that allows blades to detach freely away from the working face of the core” (1996: 294). (5) “Cores must be supported in a manner that accomplishes all of the above, and that also enables the core to be rotated and maintained easily” (1996: 294). Failure to respect these requirements results in various kinds of suboptimal outcomes. Wilke experimented with microcores made of obsidian and heat-treated chalcedony, flint, and chert. He explored different ways of holding microcores, including holding them on a leather pad in his left hand, various kinds of handheld clamps, and slotted blocks of bone or wood (1996: 293). Over a 3-month period of intensive work, he reduced about 125 experimental cores (June 17, 2009, personal communication). “After much experimentation, it was determined that bullet-shaped microblade cores were effectively reduced using a simple hand-held device that enabled several technological constraints to be accommodated at the same time” (Wilke 1996: 293–300; cf. Pelegrin 1988: 42, 2003: 61). Cores were bullet-shaped by the end of the process because of the way they were processed, but they were not all originally this shape (P.J. Wilke, January 30, 2010, personal communication). As with Sollberger and Patterson’s study, Wilke considered blade replication at the level of populations of related products and not of single specimens. It is the consistency of regular blades in predominant frequencies that indicate pressure manufacture.

3.2.18 Andrei Tabarev: Wedge-Shaped Microcores and Holding Devices

Tabarev received flintknapping instruction from Wilke and Quintero at the Lithic Technology Laboratory at the University of California, Riverside (1997: 143), so it is not surprising that his study of wedge-shaped microcores from North-East Asia shares structural features with those just outlined for the Middle East. Tabarev came to these experiments with archaeological background and questions about specific assemblages. In 1995, he was able to view a video which shows Rob Bonnichsen (Bonnichsen et al. 1980) making microblades with a complicated, two-man system for immobilizing cores (Tabarev 1997: 139). Tabarev’s experiments attempted to simplify this process. For the most part, he followed the production sequence outlined by Flenniken (1987), but with the suggestion for a different and more efficient hand clamp for holding cores (Fig. 3.20).

Tabarev reduced 70 experimental cores of obsidian, jasper, flint, and flinty tuff (1997: 142) in a three-point holding device modified from a Callahan type clamp (cf. Fig. 3.10). Few details of Tabarev’s experiments have been published other than to confirm that cores can be secured for blademaking with simple hand clamps.

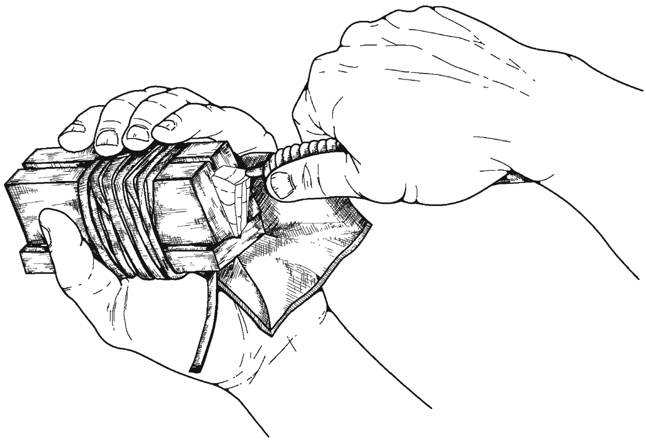

Fig. 3.20 Hand clamp for securing wedge-shaped cores (Redrawn from Tabarev 1997: 146, Fig. 5)

"During the experiments it was apparent that, for the device used [Fig. 3.20], a subtriangular preform would be best" (Tabarev 1997: 143). Of interest was the relationship between Tabarev's holding devices and initial core sizes and shapes, with it "often necessary to adjust the configuration of the microcore in order to fasten it more securely in the clamp device" (Tabarev 1997: 141).

3.2.19 Marc Hintzman: Mesoamerican Microblades

Hintzman (2000) followed the lead of his Master's thesis advisor, Wilke, and conducted experiments to understand obsidian blade production from small cores from a secondary site in the Maya Lowlands of Belize. Hintzman suspected that artisans at the site he investigated got "second-hand cores" – cores worked to near exhaustion elsewhere and imported to this site (Hintzman 2000: 23). These cores were used in the equivalent of a microblade industry.

Minimal details have been provided so far on Hintzman's experiments. He explored three techniques for holding cores that corresponded roughly to those described by Crabtree, me, and Wilke. Wilke designed a larger version of his slotted block device to hold larger cores, and Hintzman used a similar jig in his experiments (Fig. 3.21). Foot-holding did not fare well in these experiments and appears to have been quickly abandoned. Hintzman found he needed a rest for the distal end of the core for foot-holding, but even then the results were unsatisfactory. In contrast, blades made with cores wedged in a self-tightening, slotted device resembled those found at the archaeological site. This device "allowed cores to remain upright, so the blademaker could stand during the reduction process. The result was that essentially no work was required of the blademaker. The weight of the blademaker when applied through a

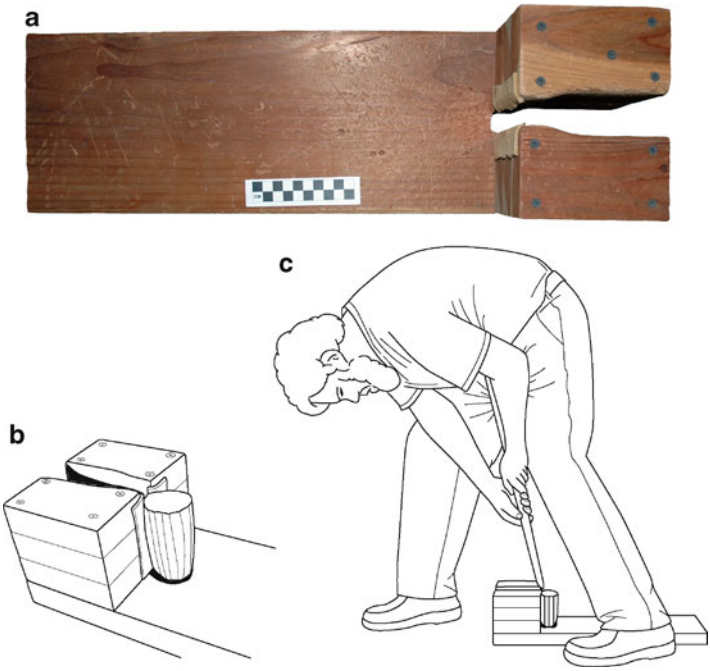

Fig. 3.21 Core self-immobilizing device designed by Phil Wilke for working normal-sized cores. (**a**) Top view. (**b**) Oblique view showing the position of a core. (**c**) Manner of working a core

pressure tool to a core provided all the energy required to detach blades. In this manner, blades frequently were produced for hours at a time" (Hintzman 2000: 39).

Hintzman brings out the issue of the relationship between core geometry and the means for immobilizing cores. His deliberations concern the benefits and disadvantages of pointed versus truncated cores. He observed that many archaeological cores have crushed distal ends, a possible indication of the use of hard supports for cores during blade manufacture (2000: 39). This is a common observation (see Barnes 1947b: 109). Hintzman's speculations would be greatly helped if his experimental cores showed similar kinds of damage; none is mentioned.

3.2.20 Peter Kelterborn: Blading by Double-Lever Machine

Kelterborn has long pursued a rigorous approach to stone-tool replication (1987, 1990a, 1999), beginning with a study of the famous *livre-de-beurre* technique of removing long blades from flint cores from Le Grand Pressigny, France – perhaps by indirect percussion (1980, 1981b) or "lever pressure plus an initiation blow" (1981a: 22). After impressive studies in replicating Gerzean knives (1984) and of debris from an axe workshop from Lake Zürich (1992), he returned to his interest in very long blades, beginning with a detailed description of a flint blade 43.3 cm long from Varna, Bulgaria (1990b). In November of 1992, he was able to spend three hours at the Field Museum of Chicago and examine a large collection of cores (December 12, 1992, personal communication). Kelterborn visited and worked with Crabtree and Titmus on several occasions, beginning in the 1970s and learned much from them (1981b: 15). More recently, he was the driving force behind the Penn State Obsidian Conference in 2000, and he brought out the best in all of us. His chapter in the conference volume (2003) describes meta-experiments. He mentions experiments in making obsidian prismatic blades, but these were to demonstrate the utility of "measurable flintknapping" and "detachment machines" as research tools rather than to elucidate blademaking per se.

Kelterborn hopes to define performance characteristics of various techniques and discover law-like relationships between force application and microattributes on blades, flakes, and cores. His insights derive their credibility from the validity of his meta-experiments, so it is appropriate to accord them some attention. His work provides a reality check on what experimenters have been up to. The cold fact is that few of the "experiments" in blademaking summarized here would qualify as experiments in a scientific venue (see Kelterborn 1987, 1990a; cf. Callahan 1985: 23, 1995c, 1996a, 1999c, 2006b; Saraydar 2008); most have been ad hoc exercises of exploration. Most "experiments" I am aware of were to cure curiosity, with little thought of formality or of eventual needs to describe or illustrate one's efforts in print. Insufficient attention has been accorded to goals, research designs, repeatability, measurable outcomes, and thorough analysis.

As described here, knappers have devised numerous appliances and aids for making blademaking easier. Kelterborn's machines and slab cores are logical extrapolations of Crabtree's use of clamps and sawn cores, as well as the lever jigs created by Sollberger. One problem apparent in old experiments is the difficulty of controlling one variable at a time. Kelterborn does this through machine knapping of standard core forms treated in standard ways. The experimental approach allows him to measure different knapping forces and to replicate consistently his results. He also controls the setup conditions before detachment that may leave no trace on the cores and blades, such as direction, distances, and angles (Kelterborn, this volume). His procedures are "mechanically transparent," repeatable, and predictable (2003: 121).

Before the objective knowledge derived from machine knapping can be of use, however, it must be demonstrated to be analogically appropriate to ancient techniques. Insights from materials science and fracture dynamics have long been associated with the analysis of stone tools, but these insights have yet to take hold – largely because many of them do not make sense of the knapping experience. For example, reading about steel balls dropped on glass prisms has never helped my knapping stroke (cf. Speth 1972, 1974). Crabtree (1975b: 4) often talked of Alaric Faulkner's (1972) dissertation work on fracture mechanics and how these experiments did not approximate the forces involved in making pressure blades. Crabtree wanted to see much more work along these lines.

Kelterborn is a structural engineer as well as a knapper, and he has taken precautions to get things right. He devised a double-lever device to duplicate the human forces involved in pressure blade detachment (Kelterborn, this volume). To test the adequacy of this machine, he made duplicates of obsidian prismatic blades and a Gerzean knife and compared them to the real things. He found no "diagnostic differences" between his possible replicas and the artifacts, so he concluded that "the double-lever machine is a suitable replacement for, and adequately reproduces, what the human body does at the moment of pressure detachment" (2003: 124). Kelterborn compared blades he made with blades made by Titmus with the Mexica tool, and there was no significant difference between them (2003: Figs. 8.5, 8.7).

Kelterborn's next experiment evaluated the adequacy of experimental cores used for deriving replica blades, but this question was entailed in his first experiments. He used cores of thick glass or cut obsidian, as long rectangular slabs ("plank-edge cores") or as blocks that could be worked into prismatic cores. His plank-edge cores relate to a Type A system and are an obviously efficient way to make many blades without having to reset or change conditions. Blades that removed the frosted surface of cut glass or obsidian are not replicas, but those taken from the active keel of these thin cores are.

Kelterborn's third experiment exposed the inadequacy of many previous studies in machine knapping. He compared the manufacture of prismatic blades of trapezoidal cross section to what is (confusingly) termed "blades" in the experimental literature; Kelterborn calls these glass prisms "long rectangular detachments." These are more akin to long burin spalls removed from thin plate glass than to prismatic blades (Kelterborn 2003: Fig. 8.10, footnote 5). Scientific knappers have tried to approximate the forces of blademaking by removing these blade-like (in length and width, but not in cross section) plates of glass (see Faulkner 1972: 98; also, Bonnichsen 1977; Tsirk 2009). Kelterborn established that there are sufficient significant differences between the two products under similar conditions of fracture that one is *not justified* in interpolating the mechanics of blade manufacture based on experiments with forcing off tabular pieces of glass. The differences in "core" geometry are too significant. From this, Kelterborn proposes his Similarity Rule: *"Qualitative fracture laws in flintknapping, and in particular quantitative equations, are only valid within the limits of strictly similar detachment attributes*" (2003: 129, original emphasis). In terms of specifics, "it means that ordinary flakes without dorsal ridges [e.g., from plate glass] cannot be used to describe flakes with one or more ridges or that flake removals with hinge or step terminations cannot be compared to removals with feather terminations" (2003: 129).

Most of us making Mesoamerican obsidian blades have claimed to have "replicated" blades (following Crabtree), but this is an abuse of the concept because no study undertaken to date has even attempted replication, according to the loose guidelines specified by Flenniken (1981b; see also his comments in Callahan 1978b). Kelterborn (1987, 1990a, 2003) provides a higher standard for scientific replication to which we should aspire. What stoneworkers have generated is *savoir-faires*. This is knowledge beyond the capacity of Kelterborn's approach; his experiments favor *connaissances* (see Kelterborn 2003: 123).

Kelterborn (2008a) is compiling a virtual assemblage of expended Mesoamerican obsidian blade cores to guide parameter research. His longest documented archaeological cores (macrocore class, 18–24 cm long) appear to have come from Michoacan (see Hester 1978) and to be the sorts of exhausted cores imported into Xochicalco for recycling into microcores (above). Some of these exhausted cores are not worked all the way around their perimeter, and they evince little evidence of plunging blades. They average 15–16 blade removals in their circumference. These cores show evidence that the "detachment technique by pressure clearly aimed at a consecutive blading order" rather than an "occasional blading order" (2008a). The sequence of blade removals may be telling about the techniques used for immobilizing cores, whether by hand, three-point rests, feet, lateral clamps, or end blocks (2003: 127, Fig. 8.9, 2008b).

3.2.21 Gene Titmus: Mexica Blading with Wooden Tools

Although he began blading soon after Crabtree did, only recently has Titmus committed some of his insights to the written record (Titmus and Clark 2003). Titmus started making arrowheads on his own when he got out of the U.S. Air Force and moved to Shoshone Falls to work for Idaho Power Company. He found many arrowheads as a youth and in his eventual work, and he was curious as to how they were made, so he set about learning how it could be done (Callahan 1980c: 21; Crabtree 1980; Hall 2002; *South Idaho Press* 1981). He met Crabtree, who lived nearby, in 1959, as a person who shared his interests. Titmus had been knapping for about a year and a half before this acquaintanceship (Callahan and Titmus 1999: 66). Crabtree encouraged Titmus to develop his own knapping technique and refrained from giving him instruction (Titmus 1981). By the time they met, Titmus was adept at pressure flaking bifaces; he learned about percussion from watching Crabtree work (May 2009, personal communication).

Titmus's blademaking efforts are of particular interest in evaluating practitioner knowledge because he has the most varied and extensive experience. Of all knappers, he knew Crabtree the longest and the best, and he helped Crabtree with his Folsom point and blade experiments (Fig. 3.22). Titmus has made thousands of blades the chest-crutch way (Callahan 1980c: 24; Crabtree 1980), fluted scores of Folsom points with either indirect percussion or pressure (Callahan 1980c: 23; Titmus 1979, 2002), and produced over 5,000 blades the Mexica way (Titmus and Clark 2003: 86). Crabtree (1980: 21) was particularly impressed with Titmus's "skill in detaching Meso-American blades by pressure." Crabtree introduced me to Titmus in 1979. Titmus observed Crabtree make blades on occasion but had moved away from southern Idaho in 1961, so he did not have much contact with Crabtree for 10 years. Titmus saw me work with an itzcolotli at the Pachuca Conference, and I demonstrated the Catlin technique on his lawn a few years later. Titmus pursued the Mexica technique out of basic curiosity: "One day I just decided I had to see how this damn wooden thing [itzcolotli] worked" (May 2009, personal communication).

Fig. 3.22 Don Crabtree (*left*) and Gene Titmus (*right*) in Kimberly, Idaho, 1979 (Author photo)

As evident in his commentary on my analysis of blade workshop debris from Ojo de Agua, Chiapas, Titmus had experimented with the Mexica technique by 1986:

> I have limited experience making blades Aztec style … I was more than a little amazed at how little inward pressure is needed to remove a blade – it's almost all upward (outward) that is needed. I could make blades travel through irregular surfaces that using the Crabtree method would have stepped the blade off. The blades also follow a very curved surface and still remain the same thickness the length of the blade, not so with Crabtree's method. The blades also look like aboriginal blades – Crabtree's method always produced blades that did not look like aboriginal blades that I have and have seen. I still can't visualize how I can hold the core immobile with my feet.
>
> Back to plunging blades. Using the Crabtree method you can produce plunging blades at will anytime your core diameter gets down to about 2 inches and under. All that is necessary is to set the tip of the crutch quite a ways back from the margin, have the angle of the crutch in line with the face of the core (or the top of the crutch tipped slightly away from the face of the core), apply almost all downward pressure, and you've got it. Thinking about this in terms of the Aztec method and in terms of a skilled craftsman, it is hard for me to believe that a plunging blade would be a common mistake, even when core dimensions are very small.
>
> (Titmus letter, February 24, 1987)

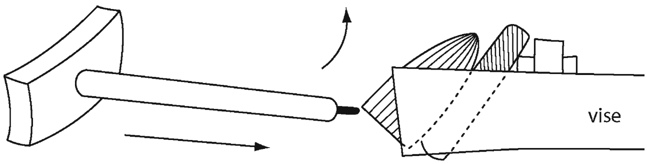

Fig. 3.23 Titmus's experimental method of holding a core for use with an itzcolotli (Redrawn from sketch)

This was wonderful and insightful commentary that I eventually answered (letter to Titmus, June 18, 1987): "You have hit upon the major weakness of my experiments. In my limited experience with the Aztec technique I have yet to produce a plunging blade. I am presuming, however, that this is a function of limited experiments [and experience]."

Just before Halloween of 1987, Titmus sent me a note and a sketch: "I made some blades last night – just wedged the core in the end of the vise and supported it in the back with a piece of wood [Fig. 3.23] and boy did the blades fly just with arm pressure. Will practice your technique come spring." Soon after, Titmus had resolved the problem of foot-holding and was making wonderful blades. As he describes it, his first attempts at blademaking with foot support for his core were "like trying to thread a needle with boxing gloves" (G.L. Titmus, May 2009, personal communication). Eventually, he mastered one way to use an itzcolotli tool.

Titmus made blades with an itzcolotli for more than 10 years before the Penn State Conference, where he demonstrated and explained his technique to colleagues (Titmus and Clark 2003). In a real sense, his experiments are of a different sort than the rest reported here. His was a long-term effort to gain proficiency rather than to check feasibility. Titmus was following a dictum we often heard from Crabtree that understanding comes from mastery, and mastery from work: "From one core what you need is 500 cores, and you start knowing a lot more about it. And after you've done 500 cores then you start admiring what the aboriginals were able to do" (in Clark 1989a: 133). Titmus is still well short of 500 cores, but over the years, he has experimented with the size, composition, and configuration of the itzcolotli tool and the flexibility of its handle (cf. Pelegrin 1984b) and with the materials and configuration of the working bit of this tool. He has experimented with copper, antler, various kinds of hardwood, and plain and notched wooden bits (Titmus and Clark 2003: 89). He has also experimented with working positions and force applications (knapping gestures and techniques in French terms [Inizan et al. 1992a; Pelegrin 2003: 57; Tixier 1967; Tixier et al. 1980]), and with the sizes and shapes of cores, especially platform and distal preparation. Along the way, he also paid attention to what he produced and why. He noticed signal features of different techniques with different tool bits. Cores used in his experiments were from three kinds of Oregon obsidian (Titmus and Clark 2003: 89). For most of them, he sawed the platform, but he also reduced scratch cores with plain facet platforms; he also conducted one experiment with a core with a pecked and ground platform (Titmus and Clark 2003: 91–93).

Titmus developed his own way of using an itzcolotli that requires a special way of immobilizing cores (Fig. 3.24). He uses a backstop and a fore-stop and wedges

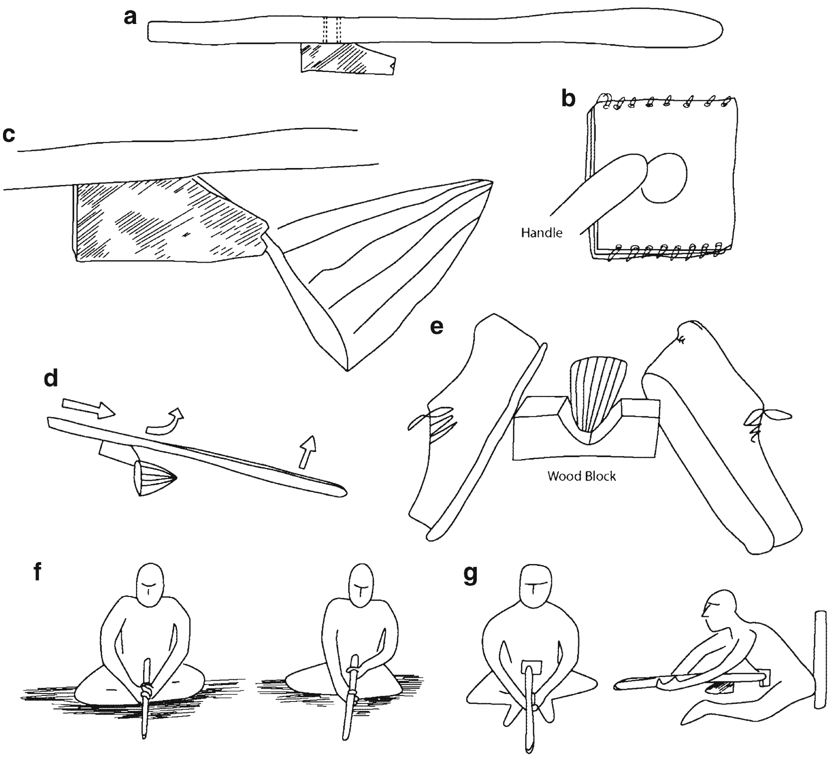

Fig. 3.24 The method used by Gene Titmus to immobilize cores with his feet (Sketches by Rob Fergus, see Titmus and Clark 2003: 81, Fig. 5.5)

himself between them (Titmus and Clark 2003: 81–85, Figs. 5.5–5.11). This compresses his body like a wound-up spring. With a strong, pole fence bracing his back and a small, notched board partially buried in front of him, Titmus places the distal end of his core in the notch of the fronting board and wedges the core in place. With his legs and feet, he applies pressure to the core from the top down, sufficient to keep the proximal and platform end of the core from moving too much during blade removal. Blades are lifted from the upper face of the core. Titmus places the butt end of the pressure tool against a thick leather pad on his abdomen, the distal end of the wooden hook of the tool contacts the upper edge of the core between his feet, and he places both hands on the shaft of the tool. He pushes and lifts/pulls at the same time, and if he does it correctly, a blade comes off up and does a lazy pirouette and lands softly on the ground just beyond his feet. If he forces things, blades fly off the core, and in front of him, and many break as they come off the core (cf. Clark 1982: 372).

The shapes of Titmus's exhausted cores make me think there is more to learn for the Mexica technique. Years ago, Titmus gave me four reduced cores and their blades. I had a graduate student glue the blades back on their cores so we could study the blades in sequence (Figs. 3.25, 3.26). One thing apparent in the exercise so far is that Titmus's cores are bullet-shaped (widest at the platform and tapering to a pointed tip), an infrequent form for expended cores in Mesoamerica. As evident in

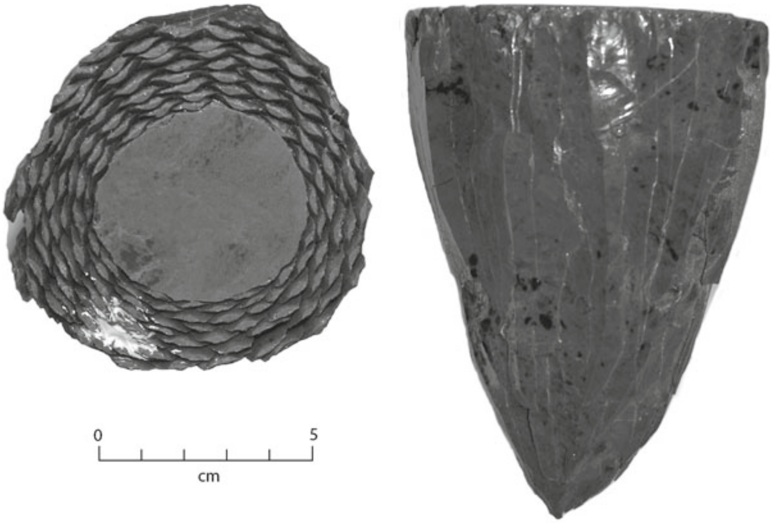

Fig. 3.25 Top and side views of a reconstructed core reduced by Gene Titmus with an itzcolotli tool. Note the regularity in the size of the individual blade platforms

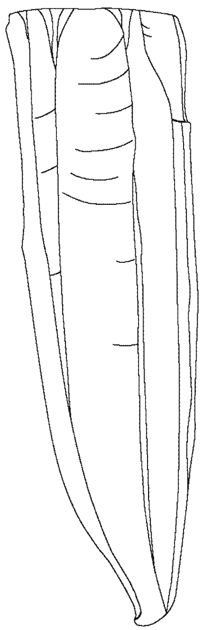

Fig. 3.26 Exhausted obsidian core reduced with itzcolotli pressure by Gene Titmus. Note the hooked distal end where the core was supported on the wooden stop block

one of my experiments, blade platform angles and the shape of a pressure core can change with ring position (Fig. 3.8). The blades Titmus makes from his cores do not exhibit this regular transformation in platform angles. Rather, his cores maintain about the same platform-to-face angle from beginning to end. I have seen a few archaeological cores like these (Crabtree 1972e; Holmes 1919: Fig. 98, center and lower right), but most are widest near the distal end (see Holmes 1919: Fig. 98, left). Titmus generates more force with his backstop and end block than I do without them. He also uses a 75° angle that requires such force rather than my 105° approach. I suspect these inputs account for the differences in the geometry of the expended cores we both produce with the same tool. What I surmise from these differences between Titmus's experimental specimens and Mesoamerican artifacts is that his technique, for all its marvelous successes, still lacks something. My suspicion is that it has too much extra, namely, the backstop, end block, and surplus energy.

3.2.22 Greg Nunn: The Blade Spectrum

Nunn is a highly skilled knapper best known for his replicas of Type IC Danish daggers (2005, 2006). He has been making and learning about percussion, indirect percussion, and pressure blades of flint and obsidian for about 17 years (2007a, b) but has not published on his blade experiments, presuming that his observations must have been made by predecessors (July 2009,[1] personal communication). His knapping history shares features with most American stories presented here, with a few unusual twists. He started serious knapping in 1986 at the age of 30 and began replicating Type IC Danish daggers in 1992, but his interest in knapping and making things goes back to early grade school when he watched his father try to make arrowheads by heating flint chips in an oven and then using a feather to drop cold water on these heated flakes. A knowing neighbor informed them that Indians used antler to flake arrowheads rather than heat and cold, and his father then tried this technique with some success. Nunn remembers studying intensely Bordes's photo-essay in *Early Man* while in fourth grade and being impressed with Bordes's percussion techniques – but not attempting them at that time. His family moved to a ranch on Wilson Mesa in the La Sal mountains east of Moab, Utah, in 1971. There, he found arrowheads and other artifacts during routine work, and he experimented in trying to make duplicates of them from the tough local agates and chalcedonies available. In 1986, he became serious with understanding how these tools were made. He discovered Crabtree's published works and studied them carefully and was disappointed to find that Crabtree was no longer living. Nunn's self-education concerned pressure flaking. He started learning basic percussion skills in 1988 at Callahan's Cliffside workshop in Lynchburg, Virginia. In 1992, he was Callahan's assistant at the dagger-making workshop held at Glass Buttes, Oregon. Titmus visited this workshop and demonstrated Aztec blademaking with an itzcolotli tool and foot-holding technique.

[1]All dated comments in this section refer to information communicated to me by Greg Nunn.

Nunn's learning experience with blading is particularly informative because he was able to observe and work with some of the best knappers in the world, but at the end of the day, he still had to figure out critical details of techniques he observed. His learning cycle has been to observe, try the witnessed technique, and then work out problems not appreciated during the primary observing. Nunn began by exploring pressure blades and then later moved to percussion blades. What he read and saw had to be translated into personal, embodied knowledge. Nunn was and is motivated by curiosity and the joy of learning through doing. In the space of a few years during the early 1990s, he was able to watch Callahan, Bradley, and Titmus make pressure blades of different sizes and materials. Nunn relates his beginning experiences as follows:

> My first exposure to blade making was watching Errett Callahan doing a short demonstration removing obsidian microblades using a forked stick and a hand-held pressure-flaker (1988). My next exposure was Bruce Bradley (1989) who demonstrated to me at his house [in Cortez, Colorado] how to make hand-held pressure microblades without a holding device. He and I were both making nice pressure blades from flint and obsidian that day. I really like this approach because a holding device was not necessary, and I can make extremely nice blades this way.
>
> (July 23, 2009)

Nunn met with Guiria and Bradley in 1990 at Bradley's house in Cortez, and he watched these two make giant pressure blades with lever pressure, with the longest blade measuring 28 cm in length (July 23, 2009; Migal 2006: 395 lists his longest blade as 27 cm). Nunn did not participate in the experiment but did provide some tools for the project. He subsequently tried this technique. For the Cortez experiments, large cores were self-immobilized in a slot cut into the side of a tree; there was no distal support for these cores. In his own experiment, Nunn started by having a friend haul the trunk of an old cottonwood tree to his property so he could make the slotted tree system needed to immobilize large cores. With the help of another friend, Bruce Mace, Nunn was able to perform the two-person lever pressure technique described by Volkov and Guiria. The pressure tool used was a pick handle of hickory 90 cm long, with a fashioned bit of moose antler "off to one face of the handle. This offset keeps the wood handle edge from contacting the core while the blade is being removed. If the bit is centered, the wood will contact the core too much, often damaging the tool" (July 25, 2009). For a lever, they used a log pole 2.6 m long. A rope connected the pressure bit to the lever. The hole in the pick handle pressure tool was next to the bit. The locations of the rope on the bit and on the lever are both significant, and the whole system has to be "tuned" (July 25, 2009). Of special interest, his friend operating the lever "did not know a thing about flintknapping. Under [Nunn's] direction he was able to operate the lever and, at the same time … create a dialog of communication to perform the process. Throughout that year [they] made blades that would be the equivalent to the total [work] of two weeks or so" (July 24, 2009). They were able to make obsidian blades about 25 cm long and 5 cm wide.

Nunn's subsequent experiences centered on making blades in the middle of the size spectrum and were inspired by Callahan's and Titmus's techniques.

> I went to Errett Callahan's Cliffside workshop again (1991). He demonstrated his technique of removing nice pressure blades with his holding device, using a gut crutch and pushing blades away. He seemed to be using his chest more … He did mention that a slightly curved-stick gut crutch may help in removing blades. I went home and made the same device, exactly like Errett's, and a curved gut crutch with a copper tip. I made some small cores with percussion, then switched over to pressure using my replica vise. I was able to remove very narrow blades away from me about 5 cm long, but I could not build enough pressure to make the blades expand any wider, and it hurt my chest. While I was attempting to make blades, I was fortunate to have a very large neighbor kid there acting as a spectator. Out of my frustration, I had him step on the vise. I then walked to the front of the vise/core, placed the gut crutch on the core, and using my chest removed a very nice blade towards me! I was impressed with the instant results. I then readjusted the core and removed another blade … then, I slid the crutch down towards my belly and removed another blade with even more ease and less pain than previously successful blades. That was pretty cool and a major jump forward. What I didn't like about my success was I had to have someone stand on the vise.
>
> (July 24, 2009)

These simple accommodations made during practice and knapping drive home two important points: (1) the need to secure the vise a core is held in, and not just the core, and (2) the different energy requirements of using a crutch tool in different working positions.

Nunn's next major experiment was inspired by Titmus's demonstration of Aztec blademaking, but he also continued to work with the chest crutch system and modified his fixation system, his tool, and his bit. Rather than rely on a Callahan or Crabtree type clamp, Nunn took his inspiration from the slotted tree system of holding cores. To accommodate cores of different sizes, in 1994 or 1995, he built a "small version of the slotted log to accept small cores about 5 to 8 cm long" (July 24, 2009).

Later, he refined and multiplied his log device into a more comprehensive system by making a graduated series of sockets in three stationary logs. Nunn's current workshop has three logs of different diameters. His small log with the two smallest sockets is 11.5 cm in diameter, and the other two logs with larger sockets are 15 cm in diameter (July 25, 2009). These logs are positioned to form three sides of a rectangle 4 × 5 ft (Fig. 3.27). Each log is anchored with long bolt stock on each end to keep it from rolling, but each can easily be repositioned to change the angle of the different sockets if need be. The holes or sockets in the logs are the shape of truncated cones, being widest at the top and tapering to a flat base. On one side, they have a narrow, vertical slot to allow a removed blade to fly free, similar to the device used and independently made by Wilke (see Fig. 3.21). The truncated cones cut into the logs differ in diameter and thus allow one to work the same core in the same workshop by progressively moving to smaller sockets as the core becomes smaller (Fig. 3.27). A core rests on the flat bottom of the tapered socket and is pressed against the side of the socket with the vertical slot. Given the size and shape of the sockets, cores are not as stationary in them as in Pelegrin's three-point devices, and it is necessary to secure them by placing wooden wedges at the top to squeeze them into their sockets. Blades are then removed by standing in front of the core and using a flexible gut crutch to remove them through the vertical slot cut in the log. A piece of cloth is placed in a depression just outside

Fig. 3.27 Greg Nunn's slotted log system for immobilizing cores (Photographs courtesy of Greg Nunn)

the slotted log to keep blades from breaking when driven downward to the ground. Nunn also improved his blading by modifying his crutch tool:

> Sometime before the turn of the century I saw a photograph of Pelegrin's curved-stick gut crutch with the antler bit set into the tip at a particular angle [see Inizan et al. 1992a: 96, Figure 46; Inizan et al. 1999: 149, Figure 73; Pelegrin 1984b: 119, Figure 2, 2003: 60,

> Figure 4.5]. I noticed the stick also had some whittling – kind of like a bow – and no bark. My curved cherry wood gut crutch had bark. I then picked up my gut crutch and modified it by scraping the bark off and scraping some wood grain off with a draw knife and flint blade. This accentuated the curve. I also reset the antler tip like Pelegrin's crutch. I went outside and gave it a try. It had noticeably better performance, and I could use the flex of the stick to my advantage – much more than before.
>
> (July 24, 2009)

With his refined fixation system and crutch, Nunn is able to remove obsidian blades 15 cm long without undue force. This accomplishment is another testament of the correct application of energy because Nunn weighs 130 pounds, so the size of his blades and their ease of manufacture are not related to body mass.

Getting back to small cores, Nunn's ongoing experiments with microblades ironically owe more to watching Titmus make Aztec blades than having witnessed Callahan and Bradley make microblades with hand clamps or freehand pressure. The Titmus system centers around a different tool, gestures, and holding system, with perhaps the most important element for Nunn's current system being a notched board for holding the core with the feet (Fig. 3.24). "After Gene did his demo at Glass Buttes, I went home and built the Aztec tool. I had instant success. I made about 15 blades from a percussion core. Then everything went to pieces after that. Most likely, I wore my tool out" (July 24, 2009). The working bit of Nunn's itzcolotli tool is of ironwood and is 28.3 cm long and 4.2 cm wide, tapering to 8 mm at the very tip. His handle is semi-flexible. In response to my questions about his blading experience, he gave this technique another try, with some success. He observed, "I keep crushing my platform, and if I do not have the correct angle [the core platform] chews up the tip of my tool" (July 23, 2009). Tool maintenance is clearly an important concern with wooden-tipped tools. A few days later, he tried again and removed five 8-cm-long blades in sequence. "The core was small in diameter, and I couldn't hold it any more. I removed blades barefooted! The wood tool is so large, and the core so small in diameter that I am afraid I could run a blade, or a fragment, through my foot" (July 27, 2009). Recently, he removed about 40 fine blades in a row (August 12, 2009). Other than Titmus and myself, Nunn is the only knapper I know who has had this much success with making blades with foot-held cores.

After his earlier, declining success rate with his itzcolotli tool, Nunn blended the two techniques he was familiar with and began making pressure blades with a gut crutch in a seated position. His short crutch is modeled after Pelegrin's shoulder crutch (Inizan et al. 1992a: 96, Fig. 46c; Pelegrin 2003: 62, Fig. 4.8). For small cores and microcores, he prefers a buried, notched board to his socketed logs. Nunn holds microcores with his feet (shoes on or off, depending on the size of the core) in the manner described by Titmus. Nunn sits with his back to a large slab of rock which leans against the wall of his house. He uses a longer crutch for medium-size cores. Both crutches have curved, flexible handles and moose-antler tips.

The published record of Nunn's blading system presents a paradox to the avid reader because the two-page photo-essay of his work (Nunn 2007a) shows a different way of holding cores than the one just described. In principle, the illustrated procedure is patterned after his slotted log system and Pelegrin's tree-root system (No. 7, above). The system in the photo-essay is much more complicated than

Nunn's slotted log system. When he uses his padded crutch, Nunn stands over a core and first sets it by pressing the tip of his tool into the center of its platform. Once the core is set, he positions his moose-antler tip on the core's outer edge in the area facing the open vertical slot in the log. His working angle is nearly vertical or slightly acute. Pressure from his torso forces the tip of his tool into the core – while keeping the core immobile and also setting the tip of his tool in the crutch. Slight or moderate outward force serves to release a blade.

I visited Nunn on July 15, 2009, and he showed me the basics of his core-holding devices, tools, and blademaking techniques. I had never used a self-immobilizing means for holding cores before, so I was looking forward to seeing – and feeling – how Nunn's system worked. His tools were too short for me, but I made some adjustments in using them. In making microblades with Nunn's short gut crutch, I had to remove his stone back support to give myself sufficient room behind his notched wooden backstop to hold the core with my feet. This left my torso half a foot away from the crossbar of his crutch, so I placed it on my inner thigh and applied pressure by squeezing with my legs at the same time I pushed with my arms. I removed a few blades in this manner.

I was curious to see what properties Nunn's system might possess, so I suggested we take one of his discarded cores and take off a major hinge termination by reversing the core. The sockets in his logs taper in the same way conical cores do, so standing a core on its head works against the built-in angles of his holding devices. I was curious to see how an upside-down core could be worked. We secured the top of this core by pounding in three wooden wedges between the core rim and the open space in the back of the socket. We had previously created a small platform on the distal end of this core by removing the tip by percussion, but the end was still strongly curved, so removing a reversed blade from the core was a challenge and required extra force because the platform was significantly offset inward from the core's face. We were successful in removing a hinge termination with a reversed blade. Later we rejuvenated this same core by removing a tablet from the proximal end and creating a new platform. A few good blades and some more miscues later, we had another hinged mass to remove. For this, I kneeled in front of the core and used a 125° angle to drive a thick blade directly under the hinge termination. In my eagerness to remove this problem, I forgot about stressing the tool, and it cracked with the removal of a plunging blade. Nunn commented that I had put about three times more stress on his tool than it was meant to take.

In an earlier attempt, I tried to remove a blade without wedging the top of the core in place. With the release of the pressure, the core literally jumped out of its hole and did a back flip. Fortunately, no major damage was done to the core. Once we reset the core and battened it down with wedges, a blade came off with normal pressure. An obvious lesson from these exercises is that a practical system of making blades must be an effective combination of designed cores, holding devices, tools, and energy delivery. My inept use of Nunn's system demonstrates that it is possible to use tools and holding devices incorrectly. It is also possible to deliver force improperly or ineptly/inefficiently, and it is clearly possible to shape a core inappropriately for any given system. This is good news because it should be possible analytically to infer aspects of a total system from parts of it, such as the size and shape of beginning and finished cores or the maximum or minimum size of blades.

3.2.23 James Winn: Obsidian Pressure Blades and Holding Blocks

Winn is another self-taught knapper who has developed great skill in a range of technologies, including the manufacture of blades. He first became interested in stone tools in 1979 while residing in Oregon. A neighbor took him arrowhead hunting – a few artifacts later he was hooked. He became interested in how some of the finer artifacts were made, and he subsequently discovered D. C. Waldorf's *The Art of Flintknapping* and started learning the art. Beginning in the 1980s, Winn has progressed through different technologies. He prefers to focus on a particular technique until he reaches satisfactory levels of competence, and then he moves on to a different technique (July 31, 2009, personal communication).

Given the emphasis here on stoneworkers' perspectives, and the process of learning from various resources (artifacts, written descriptions, illustrations, movies, teachers), the pivotal points of Winn's story are instructive, as related in his own words:

> I decided to try making a simple percussion core sometime around 2002 …. I had never observed anyone making a core before. Having become quite proficient at bifacial percussion I expected that it would be fairly easy to make a fairly nice core …. I quickly discovered that it was not a simple process. The mindset is totally different than [for] bifacing, and it quickly became evident that I did not have a clue as to how to proceed ….
>
> The following year I decided to give it a go again, this time attempting to remove pressure blades off a sawed cube of obsidian. I was armed with sketches showing Crabtree's core-holding device. I can't remember where I saw them, but it showed a pair of 2 × 4's holding a core at the end, with nothing more than a rock wedged between the wood to apply pressure to secure the core [see Crabtree 1967: 72, Figure 2a]. I built a similar device. I used a copper-tipped crutch and attempted to remove blades outwardly [from the core face] as Crabtree described doing. No matter how hard I pushed the rock wedge in, I could not get the core stabilized enough, and the core would rotate in the vise and result in a failed or short blade removal. I had a few successful blade removals, but the failures won out ….
>
> Around 2005, my wife and I … visited a museum in Athens … and they had a nice display of blade cores … I spent a long time studying them and decided I had to give [blademaking] another try. Shortly after that we were traveling through Utah and stopped at a visitor's center. Inside I found the book *Mesoamerican Lithic Technology* [Hirth, editor 2003] …. This piqued my interest even more. Soon after I discovered that Don Crabtree had been videoed making blades, and I purchased the old video from Idaho State University [Lohse 2000]. This was the first video I had seen of anyone making blades, and so I decided to try using Crabtree's method again. I had the same problem as before, with the core rotating, so I decided to add a C-clamp to the sides of the 2 × 4's to further squeeze the core. This worked OK, but the time spent on wedging and tightening the C-clamp was excessive, and I thought there must be a better way. I re-watched the Crabtree video several times and then noticed something I had missed. There appeared to be a bolt rod between the 2 × 4's squeezing the core! That was left out of all the descriptions I had read of this method … So I discarded the rock wedge and C-clamp and drilled a hole through the 2 × 4's and installed a large diameter bolt. It worked well enough, but I found I had to reposition the core after each blade removal and it still took an excessive amount of time. I decided to abandon this method entirely and go back to the book, *Mesoamerican Lithic Technology*. My ideas for using a block of wood with a hole in it, as well as my crutch types, were primarily inspired by Jacques Pelegrin's [2003] article and pictures shown in that book.
>
> (July 31, 2009, personal communication; cf. Winn 2008a)

Winn's experience of trying to learn blading through reading Crabtree's (1968) article and scrutinizing his videos (Bordes and Crabtree 1969a; Crabtree 1972e) parallels my own. Things appear simple until one attempts to duplicate them from descriptions, and then descriptive deficiencies become apparent that the author did not anticipate. One lesson from the autodidactic exercises reviewed here is that all articles, descriptions, illustrations, and videos underdescribe what takes place in blade production. This is why learning from a proficient knapper whom one can question during the process is so important. Many critical features of the process are not part of discursive knowledge. Lacking live instruction, most of us have had to work out ways that give acceptable results. So far, Winn has progressed through three stages in his blading experience. He went from Crabtree vises to handheld methods *a là* Flenniken (Winn 2008a) and then switched to a Pelegrin system (Winn 2008b).

Winn's video (2008a) on cores and blades was inspired from Flenniken and Hirth's (2003) experimentally aided study of Xochicalco blades and covers direct and indirect percussion blades of flint and obsidian and handheld pressure blading. Two novelties of Winn's indirect percussion blade technique are a tall wooden anvil that he positions between his knees while in a seated position and his practice of scoring core platforms with a chert flake so blades will break at the scored mark. Both practices led to increased regularity of his indirect percussion blades and cores. For pressure blades, Winn compared the relative efficiencies of multifaceted cores with those with pecked and ground platforms. He followed the Flenniken and Hirth method for grinding platforms on a slab of rock on which about a pint of stiff, sharp slurry composed of pulverized chert flakes, dirt, and water had been placed (see Adams 2005). Winn first grinds away the salient ridges of a multifaceted platform and then uses a large chert flake to peck it flatter; he then finishes the platform with more grinding. A small core about 4 cm in diameter takes just under 15 min to prepare. The largest cores take twice as long.

In pressure reduction of these small cores, Winn uses stiff rubber pads with central grooves that allow for the removal of blades. Pads of different sizes are made of sections of old conveyor belt glued together. His flakers are Ishi sticks with blunt copper or antler tips. In contrast to Flenniken's preferred gesture of holding the core horizontally like a biface (Flenniken and Hirth 2003: 101, Figs. 6.2–6.4), Winn holds his cores vertically in his left hand and between his legs and uses his tool to muscle off blades into the vertical groove of his pad. This pad is open-ended and lacks distal support, so he protects the palm of his hand with leather. An experience of pushing a blade through a wad of soft leather made him opt for a palm protector of stiff buffalo leather, but he still has to be careful not to push blades through several layers of this leather. A built-in distal support connected to his slot would resolve this problem, such as used by Pelegrin and Wilke (above). At the time of his video, Winn (2008a) had less than a week's experience with his handheld core technique, but he found it superior to the Crabtree method. When cores get too small for his holding pad, Winn shifts to a smaller one with a narrower slot; thus, he is able to work cores down to exhaustion.

Winn's next blading experiences involved larger cores and holding devices similar to those used by Nunn (Fig. 3.28). Winn immobilizes his cores by placing them in socketed log blocks, with the size of the socket and its block varying according to the size of the core to be worked. Winn's and Nunn's systems both were independently

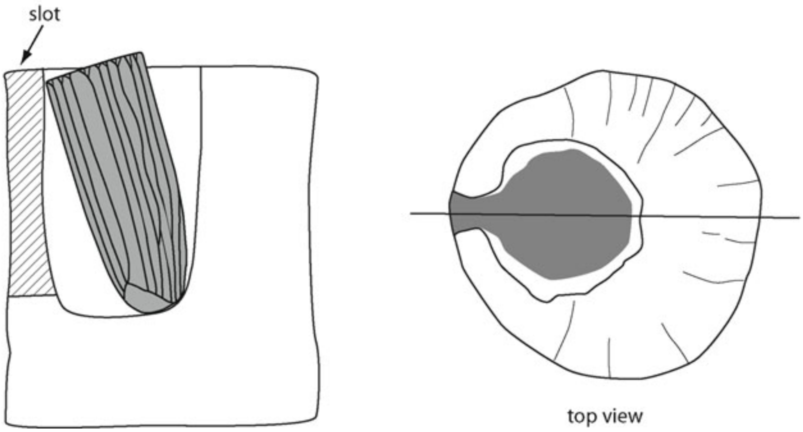

Fig. 3.28 James Winn's socketed blocks for immobilizing cores (Photograph courtesy of James Winn) a drawing showing how cores are secured in them

inspired by Pelegrin's devices (above). One significant difference is that Nunn carves his sockets into horizontal logs, so his sockets cut through the grain of the wood. Their depth is limited by the diameter of the log. In contrast, Winn uses small sections of logs cut off square at both ends, much like a section of log to be split into firewood. He then carves a socket toward one edge of the log block and cuts a vertical slot. His sockets cut into the grain of the wood rather than across it (Fig. 3.28). Some of Winn's sockets are roughly the same truncated-cone shape as Nunn's but not as carefully carved. Some of the irregular ridges of wood left from drilling within his sockets may keep his cores more stable. For other sockets, the hole flares out from top to bottom (on the side away from the open vertical slot). This allows Winn to work cores that are in an inclined rather than vertical position. Winn uses copper-tipped and blunt antler-tipped tools patterned after Pelegrin's curved, flexible tools. He does not anchor his socketed blocks; rather, their flat bases are sufficiently stable that they do not slide on a concrete surface. He can remove blades 22 cm long that require significant pressure.

Winn rarely uses wooden wedges to pin the upper part of his cores in their sockets (August 4, 2009). Cores become naturally wedged in their sockets with the application of downward force on the outside rim of the core – with the distal end of the core forced against the inner (opposite) edge at the bottom of the socket (Fig. 3.28). Once a blade comes free, the core recoils and springs back to a more upright position. Sometimes the removed blades are too wide to exit cleanly through the slot. Some blades remain in the socket with their core, with the backward recoil of the core apparently giving them sufficient space that they do not break. Some blades roll against their cores and acquire spontaneous retouch on their distal ends. The overall impression is that this is a rather simple technology and that obsidian blades are stronger than they look.

Winn demonstrates the reduction of cores of different lengths, all of them with rather small diameters. Given his graduated series of socketed blocks, he is able to work cores down to pencil size. He uses nearly a vertical angle in applying force to a core and relies on impulsive pressure, with a noticeable thrust of pressure to break blades free. For most cores, the platform is positioned level with the top of the socket, or just below it. One core, however, was about 5 cm longer than the socket depth and thus stuck out. For this core, the three points of contact were its distal end and two flanking points on the core's face about 5 cm down from the platform. This brings up the question of core length. Winn's and Nunn's systems of socketing cores attend to core diameter more than length. For short cores, one can put pieces of wood in the bottom of a socket to provide greater elevation; for long cores, one has fewer options for accommodating lengths not anticipated by socket size. How far above the rim of a socket can a core protrude and still be worked safely? Nunn can work cores that rise 7 cm above their sockets and still be safe, but he speculates that beyond this the core may be too unstable (July 28, 2009, personal communication). Winn informed me (July 31, 2009) that if cores stick up too far above their sockets, "the whole block can pivot from leverage." This is sufficient reason to stabilize the socketed block. That such stabilization is necessary only in exceptional circumstances is intriguing, given the difficulties of keeping cores stable in a Crabtree vise. Sockets cut into a tree stump would provide the best stability. It is interesting that Sellers (1886: 882, Fig. 6; also in Moorehead 1910:I: 21, Fig. 18) illustrates an analogous system. The next best thing would be to bury partially the block, or anchor it in the ground.

3.3 Blade and Core Connaissances

The major controversies raised by experiments in making Mesoamerican blades concern the distinction between *Replication* and *Technical Research*. Arguments for replication are premature because the three steps required for it have *not* been attempted in any of the knapping exercises reported here, namely: (1) careful description and specification of the assemblage to be replicated, (2) designed experiments that bracket the assemblage variability by different technical means, and (3) detailed comparisons of specimens from different experiments to artifacts from target assemblages to assess their goodness of fit. Admitting that past exercises fall short of true replication experiments does not lessen their value as technical research. As Pelegrin (1991: 120; see also Tixier 1984a) explains, technical studies skirt the uncertainty problem inherent in replication (i.e., equifinality). Technical research can proceed with certainty and establish diagnostic features of various knapping techniques, tools, and gestures. All experiments help specify the connection of specific techniques to particular outcomes. In concluding this chapter, I focus on such technical knowledge (*connaissance*). The ability to infer knapping techniques, tools, and holding devices from blades is based on understanding what happens to cores and blades during manufacture under different conditions.

3.3.1 Controlling Blade Parameters

A primary concern for technical research is the effect of human input on blademaking. What features of blades were controlled by knappers and which were not? To make this determination, one must factor out the effects of raw material and environmental conditions. Many knappers pursuing a variety of techniques have concluded that they can partially control the length, width, thickness, straightness, regularity, ridge and lateral margin character, transverse cross section, platform size, and surface features of blades. These insights started to spill forth from Bordes and Crabtree's (1969b: 6–7) efforts to duplicate Corbiac blades:

> In our experiments, generally the widths and lengths of the blades were variable and were controlled by the form of the working face of the core. The more attenuated the ridge and the narrower the core, the narrower the blade. The thickness of the blade is also controlled by the position of the punch and the design of the platform in relation to the core. The nearer the punch is placed to the leading edge of the core, the thinner will be the transverse section of the blade.

These insights concern geometry and force application and are true for all blades. Crabtree (1968: 464) records that blade "types are governed by the manner in which the pressure tool is placed on the edge of the core. The triangular blade is made by directly following one ridge, and the trapezoidal type is made by positioning the tip of the pressure tool in line with, but between, two ridges." Blades follow ridges. "Prismatic blades will be no straighter than the ridge left on one face of the core" (Crabtree 1968: 464).

Blade length is more difficult to control than width or thickness, and it also is more restrained by an absolute limit. Pelegrin (2003: 60) observed that blade length relates to the stability of the core and the flexibility of the pressure tool that removes it. Potential maximum blade length is determined by the overall length of the core from which it comes. The main obstacle in making long pressure blades is not the difficulty of pressing them off but in fashioning long cores from which such blades can be taken (Kelterborn 2008a, this volume; Titmus and Clark 2003: 93; Volkov and Guiria 1991: 382).

Blade thickness and width are related variables, and both depend on the arc or diameter of the core face, the placement of the tool in relation to this arc, and the force applied in detachment (Pelegrin 1984a, 2003: 60; Titmus and Clark 2003: 82). Some features of blades also relate to the size, material, and morphology of the tool tip used in detachment. It should become possible to identify the technical stigmata resulting from different kinds of hammers, punches, and pressure bits (see Titmus and Clark 2003: 84–85).

The most striking difference between pressure blades and those made by direct or indirect percussion is the regularity of the straight, parallel edges and dorsal ridges of pressure blades (Crabtree 1968: 457, 459; Pelegrin 1984b, 2003). For Crabtree (1968: 462), the principal problem in transitioning from a percussion to a pressure core was how to straighten ridges and begin making blades with straight, parallel sides: "to make a perfect blade you've got to have a perfect surface to work with. Any irregularity you have could affect the next blade, unless you intersect and take off a thick blade" (Crabtree, in Clark 1989a: 131). The key is to remove double-ridged blades rather than single-ridged ones because removing two widely separated ridges at a time naturally straightens the ridges on a core's face (see Bordes and Crabtree 1969b: 5; Clark 1989a: 131).

Core morphology is not the only factor behind straight blades; the actual alignment of force application is also critical (Crabtree 1968: 474; Pelegrin 1984b: 121; Titmus and Clark 2003: 84; Whittaker 1994: 225; Wilke 1996: 300). The outward force for blade removal has to be aligned with the ridges of the core or the blade removed will be "malformed" (Crabtree 1968: 476), "twisted" (Pelegrin 2003: 61; Titmus and Clark 2003: 85; G.R. Nunn, July 2009, personal communication), or "slightly … helical" (Wilke 1996: 300) due to "axial torsion" (Kelterborn 2008a: 3). Movement of the core during blading can cause a broken or malformed blade, and either outcome hurts the core. Viewed analytically, the kinds and frequencies of twisted blades in an archaeological assemblage could be evidence of the type of core stabilization involved (Kelterborn 2008a, this volume; Titmus and Clark 2003: 83–84).

The relative curvature or flatness of a blade in profile appears to be another characteristic influenced by the means of force application and core immobilization. Crabtree (in Bordes and Crabtree 1969b: 8; also, Collins 1999: 30; Crabtree 1972b: 12; Flenniken 1987: 122; Kelterborn 1980, 1981b: 12; Patten 2009: 68; Pelegrin 1991, 2006) argued that "little curved" or "straight" blades made with indirect percussion resulted from "using a rest, for it prevents movement of the core as the blades are detached and simultaneously causes force to be exerted at the base of the core when the blow is delivered on the upper end. Cores not supported by a rest will produce strongly curved blades." Wilke (1996: 294) makes a similar point for the reduction of microcores: distal core support "encourages the production of straighter blades that run the full length of the core without significant overshot." Another factor with blade curvature is that some transverse and longitudinal curvature is necessary for blade removal, especially for very long blades (Kelterborn 2008a: 3; Pelegrin 2003: 63, 2006); "if you have a little curvature, the blades come off much easier; you can give it that outward force and it will follow" (D.E. Crabtree, August 1978, personal communication).

A distal support or anvil may be apparent in blade profiles and from manufacturing marks on blades and cores (Semenov 1964: 53). For his experiments with Siberian microcores, Flenniken (1987: 121) noticed that the distal end of the "ski spall" removed to create the long platform "exhibited slight undulations, usually terminating in a small hinge as a result of the anvil use," and in other cases it showed "crushing" as well. "Frequently, a small flake was also accidently removed from the distal end of the ski spall and/or ski spall scar as a result of rebound from the anvil" (Flenniken 1987: 121). This last flake sounds analogous to impact flakes broken from the tips of projectile points (cf. Fischer 1989; Flenniken 1985; Flenniken and Raymond 1986; Kelterborn 2001). Similar flakes could result on the distal or proximal ends of a core from the use of a vertical clamp. An important caution in interpreting crushing on the margins of cores is to separate preforming features from damage incurred during blademaking. Gryba (2006: 59) argues that much "crushing" on microblade cores relates to preform preparation and not "from use of a hard anvil during blade removal."

Features of blade platforms or butts have received more attention from knappers and analysts than have distal ends of blades, and deservedly so. As remnants of the core platform from which the blades were detached, butts preserve information on the preparation of the platform surface (plain, abraded, pecked/ground, multifaceted), the angle of the core platform to its face, and the tactics for building precision into blademaking. The size and shape of the platform on a detached blade, and characteristics of both the ventral and dorsal surfaces at its proximal end, are clues to the type of force application, the tool used to detach the blade, and even the morphology of the tool bit and surface area of contact. For the dorsal surface, one notes features of the removal, or not, of overhang left by previous blade removals. For the ventral surface, of particular interest are the bulb of force and the presence or absence of eraillure flakes, lipping, Hertzian cones, cracks, concentric rings, ripples, and fissures.

As to tactics, one can build precision into the preparation and isolation of individual platforms to minimize the chance of improper contact and insufficient force application. Pelegrin describes experiments with the Levallois technique and setting up individual platforms so "you can't miss" (in Callahan 1982: 68). On the other hand, a general platform with predictable qualities can be constructed and precision built into the blademaking tools and/or their placement. Titmus induced precision by notching his pressure tool; in a similar manner, and independently, Pelegrin built precision into manufacture of indirect percussion blades by notching his punch (in Callahan 1982: 63). Another option is to have pointed tools and to be precise in their placement. Given a sufficient population of blades made by such means, it should be possible to reconstruct the platforming concept or tactic for a particular assemblage.

Platform preparation and overhang removal were under a knapper's control, and the platform angle was also under partial control. Platform sizes are a natural consequence of knapping techniques and gestures as applied through various kinds and sizes of tools (see Fig. 3.25). It is worth emphasizing that there is no necessary correlation between the size and angle of platforms and the width, thickness, length, or longitudinal curvature of blades. Rather, platform sizes relate to the type of pressure tool used and the total area of contact. Blunt wooden bits contact more surface than do metal or hard stone bits, and they result in larger platforms, all other things being equal. Pointed versus diffuse contact may relate to features on bulbs of force. Blades made with wooden and antler tools evince a lower frequency of eraillure scars and have more lipping than do blades made with harder, more pointed bits (Clark 1985; Pelegrin 2006; Sheets and Muto 1972). The locations and shapes of eraillure flakes also vary according to the type of platform preparation, with pecked and ground platforms affecting these features the most. Cracked platforms or partial Hertzian cones on the ventral faces of blades are evidence of a hard and pointed tool, such as a copper bit (Pelegrin 2006), or perhaps even flint-tipped tools (Semenov 1964: 50–54, Fig. 11). These features of a blade's bulbar area can help distinguish among the kinds of force application (e.g., direct pressure, lever pressure, and direct or indirect percussion) and the material and morphology of the tool used to remove them. Pelegrin (2006) argues that pronounced lipping is a mark of indirect percussion (see also Kelterborn 1980).

In his foundational paper, Crabtree (1968: 449, 451, 469) claimed that the small platform size of Mesoamerican fine blades was evidence that they were made by pressure rather than by indirect or direct percussion. This is not necessarily the case, as Crabtree discovered soon after in experiments with direct percussion with "edge-ground cobble" hammerstones (Crabtree and Swanson 1968). It is possible to make percussion blades with very small platforms (see Newcomer 1975: 100; Quintero and Wilke 1995; Wilke and Quintero 1994). The analytical literature related to Mesoamerican blades describes changes through time in platform size, shape, and preparation, all of which may indicate an evolution in methods and techniques of blade production. Early blades have isolated, individual platforms; later blades were from cores with platforms prepared for blade removals in groups; and the latest blades had pecked and ground platforms, with little attention accorded removal of platform overhang. Experiments conducted so far have produced credible replicas of early and middle blades, but not Postclassic blades that correspond to the descriptions of the Mexica technique. More experimentation is needed to understand platform treatments, their relationship to blademaking tools, and knapping gestures.

Platforms represent interesting attributes because they were under knapper control, but knappers were also constrained by the laws of force propagation. Platform angles vary as a consequence of blademaking itself, becoming progressively less-acute with the removal of each ring of blades (Callahan 1984: 92, Fig. 15; Titmus and Clark 2003: 92; see Fig. 3.8). For cores with unpecked platforms, removal of pressure blades becomes difficult if the platform angle exceeds 90° (see Callahan 1984; Patterson 1986; Sollberger 1986a). Pecked and ground platforms allow a knapper to remove blades with obtuse angles more easily. Maintenance of an acceptable platform angle would have been important for knappers wishing to maximize production of regular blades.

The distal shapes of blades are governed by core forms and ridge patterns, the amount of force applied during blade detachment, and whether or not a blade runs the full length of its core before termination. Cylindrical cores with truncated ends favor the production of square-end blades, and pointed cores favor the production of pointed and plunging blades. It is possible to produce a pointed blade from a truncated core by stopping it before the end of the core, but this adversely affects a core's ridge pattern. Blunt cores help prevent blades from overshooting; blade overshots are more frequent on pointed cores.

3.3.2 Blade Cores and Their Metamorphoses

Scholars tend to view blade technology as a means of obtaining a large volume of standardized items through controlled and skilled knapping. The hidden irony in this generalization is that blade production has to deal with a morphing core during the process. As Titmus pointed out, a major skill is to produce uniform blades from a core that is constantly changing:

> Pressure core diameter decreases as blades are removed, and average blade width must also decrease correspondingly …. Perhaps the most difficult part of pressure blade removal is maintaining the correct blade width relative to the diminishing diameter of the core in order to maintain side-by-side blade scar ridges that allow for continuous removal of two-ridged (trapezoidal) blades. This requires a consistent amount of force for each removed blade …. Standardization of blade width and thickness relative to core circumference helps maintain the core in a cylindrical shape.
>
> (Titmus and Clark 2003: 91)

Another transformation for most Type B cores was changing platform angles with successive rings of blades. This transformation occurs in the ideal circumstance in which a flat platform has been prepared. For most cores, there are two platform angles a knapper needs to monitor: the platform-to-face angle and the angle of the platform to the main axis of the core – a hypothetical pivot point running through the center of the core. Ideally the core platform should be perpendicular to its central axis. For cores with pecked and ground platforms, it was. For cores with single-faceted or multifaceted platforms, it was not. Cores may have concave or "dished" platforms (Barnes 1947b: 103) from the original percussion blow. Thus, changing platform angles due to core reduction had to be monitored in light of any tilt (or convexity or concavity) in the core platform during rotation around its axis (see Fig. 3.7). "When removing blades, one needs to control the angle between the pressure tool and the core platform, as well as the amount of pressure exerted on the core through the tool. Even when pressure is held constant, a minor change in the working angle can result in an error. If a knapper fails to compensate for minor differences in platform curvature [tilt], he can unknowingly alter the angle between his tool and the core platform" (Clark 1985: 9).

The most obvious changes in a core during manufacture are a reduction in its diameter and mass. Barring knapping mishaps or tactics that remove the end of a core, there is no necessary reason for a core to become shorter. The top of the core is removed faster than its middle or end because of the greater thickness of bulbs of force compared to the body of blades, so cores tend to become more pear-shaped as blademaking proceeds. This means that the longitudinal curvature of blades also changes by ring position. Most curvature is confined to the distal portion of late series blades. Platform angles increase, and the geometry of their midsections changes as a function of decreasing core diameter (Fig. 3.8; also, Pelegrin 1984a: Fig. 2; Texier 1982: Fig. 1). If one views blade cross sections as chords cutting off arcs, these arcs progressively represent a greater proportion of a core's circumference as it shrinks in diameter (see Callahan 1995a: 235; Hay 1978: 174–203, 208–214; Hay and Rogers 1978).

Pressure core preforms come with a set number of ridges, and a knapper removes these and establishes more regular ridges. Kelterborn (2008a) calculates from 14 to 16 ridges for pressure cores. With the production of trapezoidal blades and a diminishing core diameter, something eventually has to give; the number of ridges on the core and the distance between them are both reduced. This results in the production of narrower blades, blades with dorsal ridges that are closer together, some thicker blades, and occasional blades that remove an extra ridge from a core. Blades with more than two parallel ridges are expected from the final stages of the reduction sequence. During blade manufacture, cores get smaller, lighter, of reduced diameter, more parallel-sided, and even pear-shaped. These shifts lead to the concern for small cores expressed by many experimenters. Not only do knappers have to shift techniques during the reduction of a core, they sometimes have to change tools, working stances, force applications, and the manner of securing the core.

3.3.3 Core Stability

The issue motivating most experimentation with Mesoamerican blades has been core stability. Arguments about tools, bits, working positions, and force applications boil down to the problem of keeping cores still during blading. Preceding descriptions provide a range of options for core immobilization. Near the end of his life, Crabtree recognized that he had simplified the problem by thinking of cores as ideal forms. Pelegrin (2003, 2006) and Kelterborn (2008a) suggest that a better way to approach cores and blades is to group them by size class, the presumption being that cores of different classes have to be stabilized in different ways and reduced by different techniques. Kelterborn (2008b) identifies parameters for each size class and the limits of different holding techniques and tools.

One can accept the following facts as established: (1) Exhausted blade cores represent a range of sizes, platform angles, platform preparations, face curvatures, ridge numbers, and ridge regularities. (2) Pressure core preforms necessarily represented a similar range of forms and sizes. (3) The process of making pressure blades transformed cores in geometrically predictable ways, ceteris paribus. (4) Cores of different shapes and sizes presented different challenges and opportunities for keeping them stable during blademaking. (5) There are many ways to secure blade cores, and they have different benefits and inefficiencies. (6) Some holding techniques occasionally leave marks on the cores and blades so held. These facts are generally appreciated by experimenters. As pointed out by Pelegrin, Flenniken, and Wilke, one logical deduction from these facts is the likelihood that very small cores were held in the hand and larger cores were not. For analysts, the last two points are central because it should eventually be possible to identify technical stigmata for different holding devices and reconstruct from artifacts the distribution of techniques in time and space. Potentially diagnostic stigmata of holding devices include marks left on cores and blades and special kinds of knapping errors.

Experiments suggest that some damage to blades may be associated with certain holding devices. Healan (above) identified counterflaking as manufacturing marks, and he opened the prospect of looking for fine-grained evidence for contact points between cores and their supports. Most of the devices described by Pelegrin can be characterized as "three-point rests" (Kelterborn 2008b). Titmus's technique also relies on three contact points. In these techniques, a rest is needed for the distal end of a core, and additional support is required for opposed sides of its proximal end. Pelegrin's devices are self-tightening and take advantage of the force exerted through the pressure tool to lock a core in place without additional effort. Blades are pushed off the lower margins of their cores. In contrast, foot-holding counters the force used in blademaking. The feet push a core down, and impulsive pressure in the opposite direction removes a blade from the core's upper surface. The Mexica sources clearly state that blades were lifted from their cores (Clark 1982: 361).

As proposed by Healan, counterflaking results when a blade touches a vise board as it comes off its core, so this can occur on either of the two sides of the core near the jaws of the vise. Distal support of a core on an anvil rest can occasionally damage it. The hardness of this anvil might be apparent in the type of damage. For his foot-holding technique, Titmus noticed that sometimes a blade was removed that ran into the wooden support, and this encounter left a small hinge at the end of the core (Fig. 3.26). Pelegrin (2003: 63) reported lipping on the ends of indirect percussion blades made with anvil support. Kelterborn (2002) observed subtle lipping on the distal ends of some blade cores in the collections of the Field Museum. These are promising observations. There is a reasonable expectation that any part of a core touching a resistant surface has the potential to be damaged during its reduction.

Wilke's (1996) experiments stressed the need to truncate the microcores used in his slotted device to provide them support and, at the same time, to allow blades to release freely. His appliance sets the parameters for acceptable pressure core preforms, and it also determines the sizes and forms of cores that cannot be constrained (cf. Flenniken 1987). Once cores are too narrow to be pressed against the two supports flanking the slot of his device, Wilke cannot reduce them further. An obvious solution to this problem is to have more than one appliance for immobilizing cores, such as Nunn's socketed-logs system. Pelegrin (2003: 62, Fig. 4.8) illustrates a graduated series of devices he uses to process blade cores. In this case, any standardization evident in exhausted cores will conform to the size of the narrowest holding device. Cores that are too small for the device could be channeled into another system, as described by Flenniken and Hirth (2003). Wilke (1996: 300) observed that with a technique of handheld cores, one can expect blades with greater curvatures and a higher frequency of overshot blades. His observations signal the possibility that the types and frequencies of errors might be diagnostic of hand-holding techniques. Exploration is needed of the efficiencies of different core forms vis-à-vis different ways of stabilizing them (cf. Bonnichsen et al. 1980). I suspect that cores were shaped to fit specific fixation systems.

3.3.4 Counterflaking and Core Immobilization

Core geometry, manufacturing damage, and patterns of damage all come into play in evaluating counterflaking as an indicator of core immobilization. The critical junctures in core reduction sequences differ for cores held in clamps, forked devices, or with the feet. One implication of the formal transformations of Type B cores is that potential contact points on these cores (as per holding device) change during reduction. It follows that the potential for contact damage varies among techniques according to shifting contact points. As mentioned, counterflaking has been observed for blades made with a Crabtree clamp, Pelegrin's forked stick device, and for foot-held cores. This distribution disqualifies counterflaking as a marker of a single holding technique. But the overall occurrence of counterflaked blades in a reduction sequence may be diagnostic. For Pelegrin's devices (Figs. 3.13, 3.14, 3.15, 3.16), one would expect counterflaking to occur on the proximal sectors of blades where they touch the end of his forked stick or the margins of a slot. Since probable points of contact remain the same (because of his graduated series of devices), counter-flaked blades can be expected from beginning to end in the reduction process. Blades from the same core might also evince distal lipping from anvil support. In contrast, the overall pattern of counterflaking for lateral clamp-made and foot-held blades should differ because the contact points were not the same.

The variety of clamps and vises provides many possibilities (Figs. 3.10, 3.11, 3.12, 3.13, 3.14, 3.15, 3.16, 3.17, 3.18, 3.19, 3.20). A Crabtree clamp with flat, lateral jaws, such as employed by Healan, can be used with or without an anvil rest for a secured core. The main difference from a three-point rig is that opposed sides of a secured core are "slightly embedded into the wooden jaws of the clamp" (Crabtree 1968: 453), so the parts of the core in contact with the vise are more extensive than for Pelegrin's forked system. For conical cores, most of the contact that keeps them stable is at their platforms, their widest part. As a core becomes more cylindrical, a greater portion of its sides contact the vise, so the core also becomes more stable. As it gets whittled down and more straight-sided, the core becomes easier to hold with less side pressure. It also has more regular ridges. These coordinated transformations lead to the practical consequence that regular blades are easier to predict and make, thereby lessening the probability of removing a blade that would brush against a vise board and acquire counterflaking damage upon release. The scenario for foot-held cores is the virtual opposite of that for a Crabtree vise. For conical cores, the larger they are the less chance of lateral contact of blades. Cores become increasingly harder to hold as they get smaller and more straight-sided because they provide less purchase for the feet. Consequently, the likelihood of removing a blade that contacts the feet increases as a function of diminishing core diameter and length. Thus, counterflaking can be expected to occur on late series blades.

In sum, based on points of potential contact of cores with holding devices, I expect counterflaking to occur on the proximal ends of fork-made and lateral clamp-made blades. This microdamage may occur throughout the sequence for fork-made blades but only in the first few series for lateral clamp-made blades. For foot-held cores, counterflaking should occur on late series blades and in the proximal, medial, or distal portions of these blades. Distal lipping on cores and blades is not expected with my variant of the Mexica technique, but it should occur with Titmus's version. Distal lipping should be more frequent with fork-made and socket-made blades because cores are forced into a support. Considered together, these markers of contact should allow reconstruction of types of core fixation systems.

3.4 Coda

The purpose of this chapter has been to summarize contributions of past blade experiments and to establish a foundation for further research. The information presented supports a variety of conclusions, depending on a reader's background and interests. When viewed as technical research, all past experiments in blademaking have contributed to the pool of useful facts and knowledge. None of the exercises, however, rises to the level of a "replication experiment," and claims for such analytical rigor are unfounded. Replication studies are a worthy goal but are still a far way off. In the meantime, experimenters would be well-served to continue technical research with the care advocated by Callahan, Pelegrin, and Kelterborn.

In Mesoamerican studies, the missing piece has been detailed analyses of archaeological collections. Those individuals with the talent, time, and inclination to conduct experiments generally lack access to adequate collections – in reality, or virtually. In contrast, those studying collections lack the time and/or ability to conduct experiments. Treatments of chipped stone artifacts from archaeological sites rarely provide the thick descriptions, photographs, or drawings needed to design or guide a replication program. Familiarity with actual collections is necessary. Hence, prevailing conditions favor collaboration between knappers and analysts, such as exemplified by the cooperation of Crabtree and Swanson.

At the Lisbon conference that inspired this chapter, I asked Pelegrin and Kelterborn about the next generation of scientific knappers and was disheartened to learn that more are not in training. The same is true of the Americas, as described by Hirth and Kelterborn (2000: 73): "A point of serious concern emerging from the [Penn State] conference is that there is a real shortage of young, outstanding flintknappers with technical abilities and an analytical interest in indirect percussion (punching) and pressure blade research." The enthusiasm generated by Bordes, Crabtree, Tixier, Titmus, Callahan, Flenniken, Bradley, and Patten for scientific knapping has not been passed on to another generation – at least not obviously so. Our graying masters lack apprentices, and time is short (see Kelterborn 2005; Patten 2005a). There are understandable reasons for the current state of affairs having to do with the incentive structures of academia.

I think it accurate to claim that the perceived need (and/or prestige) of scientific flintknapping has declined substantially during the past 20 years. Of the reasons for this, two deserve comment. One reason may be that major questions appear to have been resolved, with lithic studies settling down and becoming a traditional specialty with prescribed methods. At the moment, descriptive methods appear to be driving the questions addressed in most lithic studies – an unhealthy state of affairs. As the preceding review of blademaking demonstrates, most major questions remain unresolved. Questions should drive methods. Crabtree (1975b: 4) observed that "knowledge of lithic technology is still in its infancy" and one could spend "several lifetimes" attempting to understand the technology of a single people. Mesoamerican studies are barely to the point that experiments can be designed on the basis of past accomplishments.

Another potential reason for the current state of affairs is more troubling, and it is the notion that flintknapping promotes analytical "conceit" and "myopia" (Thomas 1986a, b) or even a "holier-than-thou" attitude (K.G. Hirth, July 28, 2009, personal communication). Crabtree's message was that if one understands how stone tools are made, one can better analyze them. Crabtree's demonstrations were revelatory to most scholars, but the euphoria of his fieldschools has evaporated. The profession is now at a point where some analysts abstain from knapping because they believe it adversely affects their objectivity (Jim Woods, June 2009, personal communication). David Hurst Thomas's (1986a, b) criticisms of Flenniken's pronouncements on "anthropological" knapping (1984, 1985; Flenniken and Raymond 1986) appear to have been the tipping point (Callahan 1995c, 1999c: 4). Thomas chided claims from replicators that knapping experience was an absolute necessity. He argued that the most interesting approaches to stone tools came from nonknappers. Neither Thomas's nor Flenniken's conclusions follow from proposed facts. Knapping experience does not necessarily make one perspicacious or biased.

These are philosophical issues meriting serious discussion. Both entrenched positions distract and disappoint. Some stone-breakers believe bleeding over one's own chippage ought to be a rite of passage. In contrast, some untouchables promote knapping virginity as an analytical virtue. The acceptable truth behind both exaggerated views is that knapping experience changes how one sees past worlds. Thomas surely is correct on this point. One cannot understand the Crabtree phenomenon on any other basis. Epistemologically, however, neither camp has all truth or virtue on its side. Knapping experiences allow one to see things not appreciated before, but sight gained is innocence lost. Some of the most interesting questions I have been asked about stone tools have come from students lacking knapping experience. Lithic studies need variety and open dialog. This was Crabtree's main message, and I believe it to be Thomas's as well. More experiments rather than fewer are needed, but they should be theoretically appropriate, methodologically grounded, and applied to a broader range of questions than in the past (see Binford 1979). Analysts of all sorts can improve the field and help devise more and better experiments for addressing relevant issues of scholarly merit.

Crabtree and Tixier were motivated by their interest in technology and remaking things. This was my initial passion, but I was soon drawn into issues of production, trade, and political economy, and I designed simple experiments to extract metrics for reconstructing commodities, products, the flow of goods, and their consumption (e.g., Clark 1988). Pelegrin (2002) calls these "quantitative experiments." Recent theory has piqued my interest in cognition and meaning, and I think knapping experiments are apropos for studying such phenomena. Most knappers have a notion that they are in some way recreating knowledge and experience of the past (see discussion in *Primitive Technology Newsletter*, Nos. 1 and 2, 1995, 1996). My original intent in this chapter was to view the utility of replication from a perspective of phenomenology and embodiment theory (see Hodder and Hutson 2003). Of all crafts, the chances of understanding what ancient artisans knew and felt in their bones – their savoir faires – are excellent for flintknapping. The opportunity to learn how the ancients may have constructed meaning through the manufacture and use of artifacts is also good because sufficient progress has been made in mapping out some of the technical knowledge and parameters involved. In my preliminary attempt to assess blademaking know-how, I realized I had to deal with *connaissance* first, and that entangled me in the present effort to reconstruct a time line of experiments, questions, and knapper training and experiences. The potential for linking techniques and gestures to mind is a current growth opportunity of lithic experimentation (see Dobres 2000, 2010; Roux and Bril 2005). That said, we still need better understanding of basic techniques and methods, their distribution in time and space, and their costs and benefits under different cultural conditions.

Past practitioners have provided useful guidelines to follow. Avocational knapping is on the rise in the United States (Harwood 2001; Whittaker 2004), so there are more opportunities to learn knapping skills than ever before, even though opportunities within the academy continue to dwindle. There are also many more knapping guides, self-help books, films, and videos available for self-instruction. Knapping skills can be focused for the good of science, in or out of the academy (Kelterborn 2005; Patten 2005a), as epitomized by the work of Crabtree, Titmus, Sollberger and Patterson, Callahan, Kelterborn, and Patten. I have included in the references the major publications of these extramural scholars to aid any who might be interested in following their lead in pursuing questions of chipped stone technologies in innovative ways[2].

[2]Callahan (1978a), Callahan (1979c), Callahan (1987), Callahan (1999a), Callahan (1999b), Callahan (2000b), Callahan (2000d), Callahan (2001a), Callahan (2001b), Callahan (2001c), Callahan (2006a), Madsen (1988), Madsen (1993), Patten (1999), Patten (2005b), Pelegrin (1981b), Sollberger (1986b), Titmus (1980), Titmus (1985), Titmus and Woods (1986), Titmus and Woods (1991a), Titmus and Woods (1991b), Titmus and Woods (1992), Titmus and Woods (2003), Trachman and Titmus (2003), Waldorf (1993).

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