Evolution of the Hopewell Furnace Blast Machinery

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Abstract

The design of the blast machinery for Hopewell Furnace (Elverson, PA) over its smelting lifetime (ca. 1771 to 1883) is estimated by re-evaluation of existing evidence and prior historian claims. The estimated design evolution is not conclusive, but is tighter than, and partially different from, previous conjectures. Two significant components of the re-evaluation are a careful analysis of spring terminology to establish the meaning of the phrase "elastic piston spring," and a crude statistical analysis of "bellows dressing" work, which helps delineate changes in the blast machinery. The late period design is a double-acting wood cylinder blast, based on its existence to the present. It is found to be likely, although not proven, that the current blast was converted from a prior single-acting wood cylinder blast ca. 1838. This single-acting blast is found to be likely, although not proven, to have existed by 1800. Insufficient evidence was found to warrant a conclusion that a leather bellows blast was originally installed, as has been previously conjectured, although circumstantial evidence gives preference to a ground-mounted blast over an over-wheel single-acting cylinder blast as original equipment. Related to this, the conversion from a north-south to an east-west water wheel is deemed most likely to have been done prior to 1800, and the date of the relocation of the west headrace is deemed to not be relevant to wheel or blast changes.

Contents

1. Introduction

The Hopewell Furnace National Historic Site maintains and interprets a cold-blast charcoal iron blast furnace that smelted iron, with minor interruptions, from ca. 1771 to 1883. A major component of the furnace is the blast machinery ("blast" for short) that supplies the increased air mass flow and pressure required for effective furnace operation. A basic historical question is what was the design of the blast over time, which is incompletely known. Knowing this is necessary for basic furnace understanding, good interpretation for the visiting public, and as an important component in explaining changes in furnace production capacity. The purpose of this investigation is to improve knowledge of the blast design over the lifetime of the furnace. The ideal would be to have an exact design at each point in time, but there is insufficient evidence for this. As a practical matter, this investigation seeks to constrain the designs as much as possible. A simultaneous objective is to evaluate previously made claims in light of new analysis, to form an improved basis for interpretation.

The design of the final (1883) blast is known because part of it is in use today (2016), and those parts that could not be refurbished were available (as ruins) as a basis for accurate reconstruction. The problem is a lack of clear knowledge of earlier blasts. Earlier periods have assumptions and conjectures, but little evidence. There is a widely held *assumption* that the original blast was a wood and leather accordion bellows. This implies a conversion to the existing blast, possibly through several steps. However, historians made estimates for a conversion anywhere from the late 1700's to 1851. There are also claims of unspecified improvements at various times. This investigation attempts to constrain as much as possible the blast evolution up to the current design.

1.1. Methods

The primary method of this work is to re-evaluate known data on furnace operation, and in particular to make a new and more thorough analysis of data than is presented in prior reports, and to document the analysis and reasoning. This further investigation leads to both a reconsideration of prior conclusions, and the making of new and firmer conclusions. Little new data has been discovered, since extensive research into primary sources was done by park historians and archeologists, primarily from the 1930s through the 1960s.

The first step was to survey the furnace account books for relevant entries about furnace operation. Not withstanding that prior historians had already done this, this re-survey was done to avoid potential problems with errors or omissions in their previously collected data. Hence, even though there is little new data, this work still depends mainly on primary sources. The survey did turn up some previously missed data and prior errors, so there is some new data for this work.

Next, the modern reports about the "furnace group" were read, which include those by historians that describe their work and conclusions, and those by more recent NPS staff and contractors that summarize the former reports. One of the significant ones was KCFO, described in the source material section, below. Furnace data was extracted from these and integrated with that found in the current survey. There was general overlap between the current and previously found data, although the current survey produced previously unknown data, corrected some errors in transcription, and in particular, filled in omissions of detail. The single greatest type of new data consisted of details of iron production and timing, generating a better picture of furnace production rates. On the other hand, some previously found data was not relocated. This has generally been kept and is hence from a secondary source. There are various reasons for data to be missed on this round, including simply missing it in the voluminous hand-written pages, and deterioration and books gone missing since the 1930s. Since the previous extraction was done by trained historians, simple data will most likely be accurate.

In addition to furnace data, all prior conjectures and associated reasoning about the furnace were extracted from the modern reports. These are summarized in this report, and evaluated along with the furnace data.

In addition to the furnace books and reports, other sources on the period industry were consulted, to provide background information on what conclusions might be more or less likely.

The bulk of the work is the analysis of the furnace data and the re-evaluation of the prior conjectures, all with the purpose of drawing more complete and accurate conclusions about the design of the blast over time. Areas analyzed include claims about changes to the wheel and their association to changes in the blast; the use of leather and it's relationship to changes in the blast; the meaning of "bellows spring" and the implications to changes in the blast; a terminology analysis for apparent blast components and a determination as to whether these parts constrain the blast design; and an analysis of the history of repairs and other work on the blast. Of particular importance is an analysis of both the meaning and statistics of the work of "dressing the bellows," which gives significant evidence for dating blast changes.

One seemingly significant analysis that is not used is furnace production. In theory, blast design will affect production, so changes in production might indicate changes in the blast. However, other variables also affect production, and the observed production rates contain much variation, so production cannot be used as evidence for blast design changes. Blast design must be constrained on its own evidence. (However, production data is reviewed as a sanity check, to ensure that the data is not contradicting conjectures.)

A second useful analysis is not possible, that of comparing the new analysis to that done for previous conjectures. No records have been found explaining most of the conjectures: how they were arrived at given the totality of the evidence, and why certain evidence was privileged over others. This is unfortunate, since the conjectures are those of trained historians, and should not be lightly dismissed. The best that can be done is to re-evaluate the previous conjectures against the data.

1.2. Source Material

The primary source of furnace data for this work, and probably for most previous work, are the furnace books, also locally called account books and ledgers. These were named "source material" and assigned "SM Numbers." The furnace apparently did not keep formal diaries or other descriptions of day to day operation, but they did keep track of money. The books then constitute most of the surviving record of operating details. All of the surviving Day Books, Main Journals, Cash Books, Time Books and Blast Books were surveyed for data relevant to the furnace. General Ledgers for periods not covered by the above were also reviewed. Most of the time for 1800–1883 is at least partially covered by a furnace book, although some of this is at reduced detail. Some years have no extant books, so coverage is not complete. There is a document and associated spreadsheet describing the books and their relationships. This is currently unpublished, but should be available through the Friends of Hopewell Furnace, and through Hopewell Furnace itself.

Consistent with previous work, references to the furnace books are not given explicit citations in the bibliography, but simply specified by SM number and page. These can be located through Hopewell Furnace directly by SM number, with most available as scanned page images, and most of the rest available on site as microfilm.

In addition to the furnace books, there are interviews with "old timers," court records, land records, newspaper articles, correspondence and similar documents. These were not reviewed directly for this work, but they were reviewed by prior historians. Relevant data from these sources is included indirectly from secondary sources, as found in reports and notecards done by the historians and later NPS personnel.

Prior NPS work also involved archeological explorations, including digs that turned up hidden structures, such as the original water wheel pit. The archeologist reports have been reviewed, and are taken as primary sources for the direct findings therein, although not for any secondary conclusions.

The main record of data extracted from the furnace books by prior historians is "Kurjack's Chart of Furnace Operation," known hereafter as KCFO. Dennis Kurjack was an early Hopewell historian, and KCFO is the dinner table-size chart on which he summarized certain important information about the furnace's operation. (The original is in Hopewell Furnace's Museum Storage Building and also available as a JPEG or PDF file.) This was the main source used to integrate data from past historians with that from the survey for this work. The integrated results are enumerated in Section 6, where each data item includes it's source.

1.3. Non-Justified Statements

One apparent secondary source of information is a multitude of statements made by various Hopewell historians and archeologists in their reports. Unfortunately, many of these are made without any source, justification or explanation, as will be seen later with their enumeration. This is being pointed out without being able to provide a general explanation for the practice. However, some are clearly the result of personnel accepting statements by Harker Long at face value, as described next. Some other statements seem unlikely to have originated from Long, while others are uncertain. It is possible that early habits crept into later work, where a Long statement might be stated as fact, without attribution, or qualification with "tradition holds ..." This should be kept in mind when considering non-justified statements. In general, I am discounting same unless supported by other evidence.

1.4. Harker Long

Harker Long came to the furnace in 1867 at age 17 (apparently by his own statement) and was Hopewell's last manager. Long looms large in the area of historical "evidence" as past historians have apparently placed significant faith in his statements. Understanding his place is important in order to understand the apparent source of many statements made by historians that are made without any justification or explanation, with or without a "tradition holds" qualification.

Their view of Long is made clear on p. 2 of [Appleman 1936]: "It is from Mr. Long that we have obtained practically all the information in this report portraying Hopewell of an earlier day, that could not be obtained from the ruins and buildings on the ground." And "... we feel that Mr. Long has been invaluable and a 'find' in making possible the assembling of data on which a restoration might be undertaken with the assurance that it would be to a high degree accurate and reliable in its historical and cultural details." In other words, it appears that early historians effectively took Long's word for much of what he said. In addition, from p. 2, "As it is likely that the village, in its main features, probably changed very little from the period of the Revolution to that immediately following the Civil War, ..." In effect, this is a claim that Hopewell had no material changes over almost it's entire life. It also seems to have lead them to attach significant value to Long's comments about very early times.

Long wrote a short summary of his recollections [Long 1930], had conversations with the historian Jackson Kemper on August 7–8, 1936 [Kemper 1936], and had other communication with NPS personnel. In these, he relates details of the furnace back to pre-1800. However, the comments generally have no evidence. Further, he directly states (p. 3) that "All that I have written herein about the property previous to 1849 I have copied from scrap books filed in the Historical Society by the late Dr. George Hetrick. The greater majority of the material having been written by Cyrus T. Fox, also deceased." (It is not clear if these scrap books are still available.) So, lacking evidence, much of what Long says constitutes lore, some occurring a century and a half before the recounting and three quarters of a century before he came.

I find it significant that in [Appleman 1936], all statements are either based on direct observations of what was present in the 1930s, or are stated as fact, with no references, justifications or qualifications. Given his earlier statement about Long, this appears to translate to either “it is here now” or else “Long said it was so, and we accept it.” This style may have crept into later work. Later archeological work and analysis of documents added more reliable information, but significant assumptions with no source or justification have survived. It appears that many came or may have come from Long.

Note that Long is recounting details mainly at least a half century after the furnace closed, and sometimes around a century and a half before the recounting. For the earlier details, he appears to depend on scrap books of unknown quality that seem to not be available. Further, some of his statements are internally inconsistent, lending evidence to doubts about old memory. My doubts about Long’s reliability and the plausibility that unattributed statements ultimately came from him are further reason to discount such statements.

1.5. Other Notes

In order to simplify references to various periods of furnace life, the years since ca. 1770 are divided into four periods and given names. There is little documentation for the furnace prior to 1800. Except for one furnace book, there are only legal documents and letters. Years prior to 1800 are the *prehistoric period.* Many of the furnace books exist starting from 1800, in addition to letters and the like. The period from 1800–1883 is the *historical period.* Hopewell resumed “operations” ca. 1950, even if in a very limited sense (but including running the blast most of the time). This is the *modern period.* The years from 1883–1950 are the *pause.*

The blast machinery will be called just the “blast” for short. This is both for convenience and to avoid general use of the term “bellows,” which, as described below, is a troublesome term. Also, Gordon [Gordon 1996] on p. 104 states that this was period practice.

1.6. Structure of the Report

Section 2 describes difficulties with the term *bellows* that should be understood when reading this report. Section 3 is a brief history of blast machinery, both as general background, and to support interpreting the terminology of entries from furnace books. Section 4 summarizes statements by past Hopewell historians concerning the Hopewell blast. Section 5 is similar but for statements about the water wheel and headraces. Section 6 is the summary of raw data extracted from the furnace books. Section 7 describes the current blast and Section 8 describes the blacksmith shop. Springs, leather and dressing are discussed in sections 9, 10 and 11. Section 12 discusses the possible meaning of various parts referenced in the furnace books, such as stirrups. Section 13 discusses the record of work done on the blast, water wheel and related structures. Section 14 compares conjectures against basic production data. Section 15 summarizes blasts at other forges and furnaces for the general time period, showing what was known or common at the time. A general discussion and conclusions are presented in Section 16. After the references there are two appendices: a table of significant conclusions and an excerpt from the report detailing the restoration of the blowing tubs. The final page reproduces (at scale) a diagram of the furnace area from [Apple 1956a].

2. Note on the Meaning of "Bellows"

Part of the difficulty in determining blast design, and reading almost any work on furnaces, is that the word *bellows* is used frequently but does not have a clear meaning. It usage is discussed here.

Bellows seems to be frequently understood as what is more precisely known as a "leather and wooden accordion bellows," the type associated with residential fireplaces and blacksmith forges. At least one Hopewell document [Yocum 2008] apparently uses the word to mean only this.

However, bellows has a more general meaning and usage. The first Wiktionary definition is "A device for delivering pressurized air in a controlled quantity to a controlled location." This has no requirement for leather. The Wikipedia article for Bellows describes various types of bellows, including the "double-acting piston bellows," which is made of wood and apparently contains no leather. Yocum on p. 47 states that former Hopewell historian Russell Apple says that bellows can refer to non-leather devices, so this uncertainty is held by at least one prior Hopewell researcher. Other Hopewell documents, including KCFO, use bellows in its more general meaning.

Other modern authors also use a general bellows. McNeil's treatise on blast equipment [McNeil 1988] uses it to refer to different types of blasts. Pierce [Pierce 1957], writing about an 1810 furnace, uses bellows to refer to tub-type machinery. White [White 1947] on p. 10 attributes the invention of "wooden bellows" to Hans Lobsinger in 1550: "This type of blowing apparatus employed two wooden boxes, one fitted into the other in the manner of a piston in a cylinder." He implies a further complication when he states that they were called "tubs," implying that "blowing tubs" might refer to an arrangement other than the cylindrical tubs currently at Hopewell. Additional authors using a general bellows are referenced in the blast history section, and include Boyer, Daff, Raymond and Rostoker.

There are several examples of period authors using bellows in the general manner. McNeil on p. 102 quotes from a German text of the 1700's that uses the phrase "wooden bellows." Schoepf on p. 6 of Volume 2 of [Schoepf 1911], writing ca. 1784, has "The bellows are of wood ..." The 1757 Wilkinson patent [Wilkinson 1757] includes the phrase "A New Machine, or Kind of Bellows," which can be made out of wood or any kind of metal, but not leather. Cranstone [Cranstone 1991] on p. 88 quotes several 1730's account books as referring to "Cylindrical Cast Iron Bellows" and "iron bellows," these being in England.

On the other hand, Cranstone [Cranstone 2015] states that "the traditional 'double bellows' was two triangular (in top view) concertina bellows side-by-side with their nozzles normally blowing direct into the tuyere, and operated by cams on a waterwheel axle across their outer end (so parallel to the face of the furnace) ..." and that these would have been single-acting units. So this is a statement that the phrase "double bellows" more likely than not refers to a traditional leather blast with two single-acting units.

In summary, any use of the word *bellows* must be taken to mean any sort of blast, unless disambiguated. In this report, general use of the word will be avoided, in favor of plain *blast*.

3. A Brief History of Blast Machinery

This is a brief history of blast machinery, and certainly not complete. This is here partly to provide general background, and to support interpreting the terminology of entries from furnace books later in the report, and possibly in evaluating and interpreting claims about the Hopewell blast.

3.1. Early Blasts

Various early means of blowing furnaces have been described that generally predate Hopewell, but are summarized as background. Pool [Pool 1982] on p. 111 describes the use of reeds, people blowing into the fire through “straws.” Forbes [Forbes 1971] on p. 114 refers to these as “blowpipes.” Pool on p. 110, White on p. 9 and Rostoker [Rostoker 1990] on p. 72 describe digging a furnace into a hillside to collect natural wind and funnel it into the combustion area. Raymond [Raymond 1986] on p. 19 and Pool on p. 111 suggest the use of a goatskin or animal bladder bag. Raymond on pp. 28–30 describes “pot bellows,” which are foot-powered. Tylecote [Tylecote 1976] on p. 32 suggests both the goatskin and pot bellows as possibilities, but not certainties. Rostoker on pp. 72–75 describes these in some detail, calling them “bag bellows” and “drum bellows.” On pp. 115–116 Forbes also uses “skin” for “bag” and “dish” for “drum.”

3.2. Blasts Probably not at Hopewell

There are other mechanisms used in the general period of leather bellows that are not said to have been used at Hopewell. Rostoker on pp. 77–78 describes the “hinged fan bellows,” said to originate in China. This used a single moving leaf that rotated about a hinge to compress air in a box. In this blast the leaf was nominally vertical. This can also be seen on p. 88 of [Gies 1995]. The Japanese tatara bellows was similar except that the leaf was nominally horizontal. White on p. 13 and Pool on p. 113 describe the *trompe* or water blast, in which air is entrained in falling water. Rostoker on p. 78 places this under the heading “hydraulic air pumps.” Note that Overman on p. 405 disparages any blast that involves water, as such will add water to the blast air, and “The cases in which water does no harm in metallurgical operations are very few ...” [Overman 1865]. He also points out that having a waterwheel near the blast air intake has a similar effect.

3.3. Hopewell Period Blasts

The basic blast for early furnaces other than the above techniques was the leather and wooden bellows, also called the “concertina-type bellows” by Tylecote (p. 116) and the “accordion bellows” by Rostoker (pp. 75–76). White on p. 12 describes early American furnaces as having “large double bellows made of wood and leather driven by huge water wheels.” The bellows were said to be 20–25 ft in length and “several feet” in width. Note that this does not make clear whether “double” means a pair of single-acting units, a single double-acting unit, or a pair of double-acting units. Pool on p. 112 describes these as having “stout leather sides” and a length of 12–20 ft. He describes a box of rocks as a counterweight to expand the leather after it has been compressed by a cam. Bining [Bining 1987] on p. 67 shows a diagram of a typical two-unit layout, including the boxes of rocks used as counterweights. On pp. 70–71 he generally describes the equipment as “large double bellows made of wood and leather,” and quotes a size that is the same as by White above. Daff [Daff 1973] on p. 401 describes the size as “measuring anything up to 20 feet long and up to 7 feet wide at the breech.” He also states the the side leather was bull’s hide. Pearse [Pearse 1876] on p. 74 describes an early Pennsylvania furnace as being 25 ft high and with an accordion bellows that is 5 ft wide. On p. 112 he relates a Russian furnace whose bellows was 30 ft long by 7 ft wide. Additional examples of bellows sizes are given by [Swank 1892]: 22 ft long (p. 89); 22 ft long by 4 ft wide (p. 124); and at Cornwall, 20 ft 7 in long by 5 ft 10 in wide (p. 187).

The classic leather accordion bellows is described by Agricola [Agricola 1556] on pp. 362–374. He describes in detail the construction of a single-acting bellows. On p. 368 he refers to “two bellows” and on p. 369 states that “The levers are of the same number as the bellows ...” which implies the possibility of more than two. (He states that two per furnace are required, but describes the possibility of having multiple furnaces at one place.) The bottom board of the bellows is fixed on a sill, while the upper board moves. It is connected to two levers; the lower lever is depressed by a cam to compress the bellows, and the upper lever has a box weighted by stones at the other end of a fulcrum, which raises the upper board once the cam releases the lower lever. Note that McNeil on pp. 100–101 describes double-acting bellows and claims that they were first shown by Agricola, although he does not specify the page, and Agricola's section on furnace bellows is clearly describing a single-acting model. McNeil states that they were usually mounted in pairs, and were used by "goldworkers, smiths and assayers."

Tylecote on p. 106 has an interesting comment concerning Darby's development of more powerful blowing engines in order to replace the "large and cumbrous leather bellows which required 120 kg of leather annually." This claims to need over 250 pounds of leather per year, which is significant in light of the quantities (not) mentioned in the furnace data described later, although the number of furnaces and blasts in use by Darby is not specified.

Raymond on pp. 73–74 describes a Chinese adaptation of the leather bellows. It was hung vertically instead of horizontally so that gravity had little effect on its operation. Raymond refers to this as a "horizontal bellows," meaning that the movement is horizontal. He also describes some of these units as having "several linked chambers," although he does not describe what this means or how it relates to the double-chamber leather bellows where the center board is fixed. The diagram on p. 74 of Rostoker suggests multiple chambers in an accordion bellows, but this is not certain.

McNeil on p. 102 quotes from a German text of the 1700's, saying that conventional leather bellows "require careful management and are expensive to repair and besides last often not more than six or seven years." You must be "continually besmearing it with train oil or other fat substances" to prevent air leakage from thin leather or cracking at the folds of thick leather. He also states the life of wooden bellows as "thirty or forty years, and even longer" and "Polhem assures us that, when properly made, they will last a century." Daff on p. 401 also states that "the skins were in need of continual treatment to guard against cracks and holes ..." and "To prevent the skins from cracking, tallow and butter were used to lubricate them."

Rostoker on pp. 76–77 describes the "single-acting piston bellows," made of wood, with an example said to be cylindrical with a piston forcing air downward. He also refers to a "single-acting Japanese box bellows" which is presumably similar except of rectangular cross section. White on p. 12 describes a "crude" early version of blowing tubs used in America as being "little more than two cylindrical casks fitting closely into one another and moving up and down between four wooden posts." Air was blown into a "leather bag" to equalize pressure. This does not make clear whether there was one pair or more, whether single or double acting, why they are both called "casks" instead of one a piston, and how a "leather bag" provides enough elasticity to perform the equalizing function. Bining on p. 71 has an almost identical description, to the point where it seems that either Bining got it from White, or else they both got it from a common source. Apple [Apple 1956a] on p. 28 has the same description for a conjectured Hopewell blast, but gives no reason for the conjecture. Bining and Apple give the same citation (Schoepf) for the description. From the "crude" descriptor, it seems likely that White and Bining refer to a single-acting unit, similar to Rostoker, except for the inside "cask" and that Rostoker makes no mention of the equalizing bag. Rostoker quotes Mallet as describing the piston to cylinder seal as a twine made out of chicken skin and feathers.

The two-cask blast appears to originate on p. 6 of Volume 2 of [Schoepf 1911], who was traveling through Pennsylvania ca. 1784. The description is for the blast at Coventry Forge, 15 miles from Valley Forge: "The bellows are of wood and consist of two cylindrical casks fitting closely the one into the other and moving up and down between four wooden posts. The wind goes first through a leathern conduit into an iron pipe and so to the hearth. These simple bellows have the advantage that they may be set up without trouble or expense, need few repairs and should last well." This general arrangement will be referred to as the "Coventry blast" in the remainder of this report. Note the use of the term "conduit:" no claim of an equalizing function is made.

As a comparison to a forge blast, consider this description of the blast of Dover Forge, New Jersey, built ca. 1810, from pp 61–62 of [Boyer 1931]. The "bellows" consisted of two drums, 6 ft diameter by 6 ft high, each with a tight-fitting bottom that had a leather inlet air valve, an air-blast outlet pipe, and a circular "plunger" (piston), with a leather strip around the circumference. This is very similar to the current Hopewell blast, except that it appears to be single-acting, and lacking a receiver.

The Coventry blast has a similarity to a blast described on p. 401 of Daff, said to be based on British patent number 713, issued to Isaac Wilkinson on 12 March 1757 [Wilkinson 1757]. Daff describes the blast as "an iron cylinder moved up and down in a water-sealed, wooden tub, with appropriate inlet and outlet valves regulating the flow of air." Daff states that three cylinders were used to equalize the blast. A copy of the blast was apparently installed for a customer in 1759. However, the actual patent is much different. This is more similar to a reverse Savery engine, where the tank is first filled with air and then incoming water used to force the air out. The water is drained and more air admitted, and the cycle repeats. It appears that Daff was confused about something, either in reading the patent, or else was ascribing the wrong patent to the description of an unknown blast. So an antecedent to the Coventry blast may or may not exist.

A related blast, or blast design, is described by [Robbins 1986] over pp. 167–173, and includes a diagram. This originated in a letter to Thomas Russell of the Principio Company (Maryland), dated 1 July 1757. The language of the letter quotation strongly suggests that the diagram is a copy of a diagram instead of a drawing of the equipment, but this is not certain, and Robbins states that it is not clear which it is. So we cannot know from this letter whether the blast existed in 1757.

This had a waterwheel powered crank driving three beams via rods, such as those used with an atmospheric engine. Each beam lifted an inverted cylinder (closed top and open bottom) from its resting position inside a flooded water tank. Lifting was by chains, not a rod. When a cylinder was lifted, air was admitted to the space inside the cylinder and over the water. When the cylinder dropped (presumably by it's own weight, but this is only surmised) it compressed the air against the water surface, which exited through a pipe that apparently left through a sealed hole in the tank bottom. Three cylinders were used, presumably to equalize the blast. The purpose of the water was to seal around the cylinders, eliminating the need for chicken skin, leather, or other such material. Each cylinder was necessarily single-acting. The letter claimed a discharge of about 1,400 ft^3/min, which seems reasonable: if the cylinders were 5 ft in diameter with a 5 ft stroke, a wheel speed of about 5 RPM would suffice.

This would appear to be a better description of the same blast described by Daff, as it has the three cylinders the water seal. That it might have been real is suggested by Daff's claim that such a blast was installed in 1759. Like Daff, the writer of the letter in Robbins suggests that this follows the pattern of Wilkinson's 1757 patent. Robbins on p. 168 states that it is not clear if this is the device from Wilkinson's 1757 patent. Again, I fail to see any similarity with the actual patent at all.

This does not appear to be similar to the Coventry blast. It is true that a single cylinder version might be described as "two casks one inside the other." However, this blast had no need for "close fitting," and a loose fit is probably the entire point of the water seal. There is no hint of the "four posts" in the diagram nor of any leather piping. Plus, Schoepf made no mention of a water seal, although an omission cannot be considered definitive. There does not appear to be a basis for this blast having been seen in America.

Gordon on pp. 104–105 describes a similar blast being installed at the Principio Company "a few years" after 1757. He cites Robbins, p. 166, and references the 1757 letter to Russell. Robbins demonstrates that there was ultimately a cylinder blast at Principio, but on pp. 171–173 makes it clear that the "celinder" blast was not operating until 1773, and that it's design is not known. Gordon confirms that the book should have used "eventually" and is not disputing the 1773 date [Gordon 2016]. There appears to be no evidence of a cylinder blast at Principio close to 1757.

The "double-acting piston bellows" has an origin attributed to China, and is described in various forms. Rostoker on p. 77 describes both cylindrical and rectangular cross sectional ones, with the diagram on p. 74 showing a horizontal arrangement. The Wikipedia article for Bellows includes this description of the "double-acting piston bellows:" "A piston is enclosed in a rectangular box with a handle coming out one side. The piston edges are covered with feathers, fur, or soft paper to ensure that it is airtight and lubricated." The San Diego Chinese Historical Museum has a diagram of what they also call the "double-acting piston bellows," showing a rectangular box and piston, consistent with the Wikipedia description. Through flap valves, it pumps air on both the in and out strokes. Raymond on p. 74 describes the apparently similar "double-acting box bellows." Here there are top and bottom chambers with connected piston rods. One blows on the in stroke, the other on the out stroke. Pool on p. 112 apparently alludes to the same device, calling it a "piston blower." He states that it was worked by hand rather than water power, although the Wikipedia article on Bellows states that Du Shi of the Han Dynasty used a waterwheel to power a "piston bellows."

Gordon on p. 105 states that "By the 1780s most American ironmasters used such wooden blowing tubs rather than bellows." Note however that by "ironmaster" Gordon intends to include those who operate forges as well [Gordon 2016]. This references p. 63 of the 1783 Hermelin report [Hermelin 1783]. The actual quote from Hermelin is: "and at most of the hammer forges vertical wooden blast cylinders are used, and at some [of the others] wooden bellows, formerly [so] common. Several blast cylinders have been made this year of cast iron at the Cornewall blast furnace." Hermelin is describing forge blasts, which are smaller, and not necessarily furnace blasts. That most Pennsylvania area furnaces also used cylinders cannot be ruled out by the statement, but he is not claiming this, and hence neither is Gordon. However, the production of iron cylinders clearly implies a trend. Further, the phrase "wooden bellows" is not a mistake, unless it was Hermelin's. This brings up the question as to whether he considered the accordion-shaped bellows made entirely of wood to be common, instead of the wood and leather accordion bellows.

Boyer on p. 44 quotes from a journal of Cazenove, "an eminent Frenchman" who was touring the US in 1794, and who described a forge at Boonton: "Bellows of new construction, kinds of iron boilers whose lids are pushed by pistons up to the further end, and from there the air passes through tin pipes into an iron pipe which conducts the air into the fire." This is describing a multi-cylinder iron cylinder blast, but the moving "lids" imply single-acting.

White on p. 13 describes "so-called double cylinders." These "consisted of two wooden cylinders, set side by side. They blew into a third cylinder which ordinarily was weighted so as to get as uniform a pressure as possible, ..." Bining on pp. 71–72 again has an almost identical description. Neither White nor Bining says whether the cylinders were single or double acting. This approximately describes the current Hopewell arrangement, except that the Hopewell equalizing box does not have a weight, and Hopewell's cylinders are double acting. Pool on p. 112 attributes this same arrangement to the same Hans Lobsinger as White mentions, although in 1540 instead. (McNeil uses 1550, but lists additional contenders.) Note that White called the Lobsinger tubs "boxes" and made no mention of an equalizer, so Pool is effectively attributing White's more advanced device to an earlier time than White. This is interesting as Agricola apparently describes only leather and wood bellows for the mid-1500's time frame. Rostoker on p. 79 describes and shows this same mechanism, calling it the "wooden or cast iron cylinder blower or piston compressor." He does not appear to use the term "tub." His diagram is of the same style as the Hopewell blast, except that his two main cylinders are single-acting and his equalizing box has a weight.

An interesting device due to its name is the "wooden box bellows" shown by Rostoker on p. 79. This has the same general shape as the triangular accordion (leather and wood) bellows, but the sides are wood and the bottom is moved up and down inside the sides, compressing the air against the top. He states that "Such devices were widely used in Europe and North America during the seventeenth through nineteenth centuries." This adds further confusion to terminology, as others refer to a "box bellows" as a cylinder and piston machine with a rectangular cross section. Overman on p. 397, in reference to leather accordion bellows, states that "Some bellows of this form have been constructed entirely of wood, which produced a stronger blast than the leather bellows, but they have become antiquated, and are not any longer employed." This appears to support Rostoker's claim of their use. Unfortunately, Rostoker's reference does not fully support his claim. Osborn [Osborn 1869] on pp. 543–544 describes this device, calling it "Widholm's bellows," and says it "is very extensively used in Sweden, Russia, Germany, and France. We do not know that any are employed in the United States." It is of uncertain relevance to furnaces, as Osborn further states that "This kind of machine is applied to no other apparatus than the charcoal forge ..."

Plot [Plot 1686] on pp. 165–166 describes "... the famous wooden bellows that had no leather about them, ..." which is the same style of bellows as described by Rostoker, although the details are different. Plot has diagrams worthy of Diderot on Table 10, between pages 164 and 165. This looks like a leather accordion bellows, with a wood top shell that is moved up and down by the wheel, over a bottom frame that contains the valves. It is not certain from Plot what these bellows blew, as they were in storage; it is possible they were for copper smelting. Plot is describing Stafford-Shire in England, so this is offers no direct commentary on a blast at Hopewell, but if it was "famous" in England close to a century before Hopewell was built, it represents a possible blast option.

Terminology is further confused by Schubert [Schubert 1957] on p. 208: "An improvement of considerable importance was the substitution of wooden box bellows for the leather bellows ..." and "The wooden bellows or blowing tub consisted of two large close-fitting boxes, one of which was raised and lowered upon the other." Here he is conflating "wooden bellows," "wooden box bellows" and "blowing tub." His reference is to Plot (above), so this is clearly the wood version of the leather accordion bellows. This is very different from the box bellows terminology described earlier, which is just a cylinder blast of rectangular cross section. The similarity of the wording to the Coventry blast description raises the question of whether the Coventry blast was really a wood accordion blast. However, the term "casks" implies components of a very different shape, and there is no place for the "four posts" in the wood accordion bellows, so this seems unlikely.

Daff places the earliest use of "cylinder bellows" and "iron bellows" in England as ca. 1750, and in one case the cylinders were cast iron 3–4 ft in diameter, and the pistons were "packed with leather." Pearse on p. 101 states that "Wooden blowing tubs, of short stroke, about three feet, were introduced not long before the Revolution." Gordon on p. 105 states that a popular early nineteenth-century blowing tub design had tubs 5–7 ft in diameter or sometimes square, with leather-edged pistons. Swank on p. 187 states that "At first large leather bellows were used exclusively to blow both the forges and the furnaces, but afterwards, about the time of the Revolution, wooden cylinders, or 'tubs,' were also used."

Overman on p. 399 states that fans were in use (writing in the mid-1800s) but that they were only good for low pressure, less than what cylinders produced, and less pressure than what Hopewell was claimed to have used.

Overman on pp. 400–402 and his Figure 186 shows a blast that he characterizes as the "wooden blast machine most usually made." This has two single-acting wood cylinders but with no crossheads. The lack of crossheads is a problem as it means that the motion will twist the pistons within the cylinders. This is clear from the figure, and so stated: "As the movement of the beam does not produce a perfectly parallel motion in the piston and its rod, it is necessary that the stroke should be short." He states that this blast runs at 15-16 strokes/minute and $\frac{3}{4}$ PSIG, and costs \$500-600 when made out of wood and \$700-800 for iron. On p. 404 he shows a three cylinder metal blast that does use crossheads.

Cranstone [Cranstone 2015] provided a description of blast development in Britain in the 1700s. "Blowing cylinders are first documented in 1736 ..." for finery forges, which have a smaller air requirement than furnaces. He relates the first recorded installation on p. 88 of [Cranstone 1991]. These "were not widespread until after 1754 ..." Angerstein [Angerstein 2001], traveling ca. 1754, related that the Swalwell Works had one forge that had a bellows consisting of two cast iron cylinders. These were apparently single-acting, being open at the top, and used counterweights to raise the pistons. He states that the bellows were said to have been in service for seven years. Regarding furnaces, Cranstone states that “wooden box bellows do seem to have been installed at a few furnaces (all coke, I think) in the 1740s and 50s, though they were not widespread ...” and “The first clearly documented use of cylinders for furnace blowing is probably 1759 ...”

Another early cylinder blast is the subject of British patent number 783, issued to James Knight of Brindgwood in 1762 [Knight 1762]. Robbins on p. 170 credits Knight with the invention of blowing tubs, although this seems at odds with Cranstone’s comment (above) about wooden box bellows. This bears much similarity to the blast described in the letter to Russell in that beams are used to raise and lower inverted cylinders. In this case, the cylinders are explicitly stated to require sufficient weight so as to lower themselves. The difference is that instead of pushing against a water bath, each cylinder pushes against a piston that sits upright and whose rod is anchored to the ground. The piston and cylinder are of square cross section and made of wood or iron. Some complex arrangement of piston rings and special corner pieces are used to seal the cylinders against the pistons. The valves and discharge pipes go through the pistons. I do not recall reading of an actual blast that matches this description.

White on p. 13 places the earliest American use of a steam blowing engine as 1814. Hopewell installed a backup engine in 1880 (by [Robinson & Associates 2004] p. 38 and [Long 1930] p. 6) or in 1881 (by [Kemper 1936] and entries in SM 34). However, this bore no relation to the rest of the blast, since it apparently drove the water wheel, using the same remaining machinery as before. Since one of the existing tubs is said to be an original from the last blast (Yocum, p. 148), this represents the highest development in blast machinery that is relevant to Hopewell, although McNeil, Rostoker and White all continue past this point in time.

4. Blast Statements from Hopewell Furnace Reports

Conjectures of prior blast design come from official reports. The first step in understanding what is believed about blast design is to understand the statements made in these reports, and in particular, how the conjectures and conclusions were arrived at and justified. Part of this process is tracing statements back to their roots, as conclusions in later reports are frequently only restatements of those from earlier reports. Once the unique statements are known, their justifications can be evaluated. This section summarizes these statements and their justifications.

4.1. The Initial Blast

Statements about the initial blast machinery are:

- The Robinson & Associates report (R&A, [Robinson & Associates 2004]) on p. 24 states: “Like many charcoal-fueled iron furnaces in the eighteenth century, Hopewell Furnaces initial blast machinery consisted of wood and leather double bellows.” The only reference is to the Hugins report (later in this list).
- R&A, p. 37, referring to the initial furnace, states: “The machinery operated by the wheel that provided the blast of air was conjectured by park historians Dennis Kurjack, Walter Hugins, and Russell Apple to have been a large leather bellows, or pair of bellows, situated north of the furnace.” The emphasis here is on the word *conjectured* and the uncertainty whether there were one or two. That they do not rule out having only one unit itself indicates a lack of evidence, since all furnace leather blast descriptions are for two units.
- [Apple 1956a] on p. 28: states: “It is thought that from 1770 to circa 1805, that leather bellows, located under the bridge house and powered by a north-south water wheel, provided the blast.” No justification is given, and the reference is to a sketch of a conjectured blast instead of a justification for it.

- [Apple 1956b] p. II-119 quotes Kurjack as stating that the initial blast was a wood/leather accordion one, using "one or more large leather bellows ..." The reference is to Kurjack's June 1949 report. However, this report gives unsupported conjectures about later blasts, and does not specify or justify the initial blast.
- Donohoe [Donohoe 1956] on p. 4: says that the blast is "believed" until around 1800 to be a "crude set of double bellows" with a 30 ft north-south water wheel.
- Cass [Cass 1952] on p. 1 states: "Tradition has it that the first Hopewell Furnace water wheel was a 30-foot overshot wheel running north and south, or at a right angle to the furnace stack. This tradition was verified in part during the summer of 1951 when archeological investigation uncovered a north-south wheel pit and forebay pier located north of the present wheel pit." No reference or justification is given.
- Cass on p. 2 states: "It is likely that the first blast machinery at Hopewell Furnace was a large double bellows made of wood and leather, typical of the 18^{th} century." No reference or justification is given.
- Hugins [Hugins 1954] on p. 9 states that "traditionally" the initial blast used a 30 ft north-south wheel and "double bellows of wood and leather". He references the above statements by Cass and also pp. 21–26 of the archeology report [Schumacher 1951], which does not provide any justification for this. He also cites Harker Long.
- Schumacher on p. 21 states that "According to records and conversations with 'old timers', there was, prior to 1800, an overshot wheel with a 30-foot diameter placed in a north-south position." No records are referenced and no justification is given.
- Schumacher on p. 41 describes what is called the north furnace room, where the blast pipe descends from the receiver to the tuyere. He states that "Prior to 1800 the north-south wheel hub probably extended into this room, and a pair of bellows were operated on a trip hammer arrangement by rachets." No references or justification are given. The ca. 1880 boiler and engine were in this room, and they found the masonry foundation for same. He also states that "Other bits of true and dry masonry foundations were located scattered in the room. These seemed to be of an earlier period than the foundations of the boiler and engine. Possibly they had been used prior to 1800 as supports for the bellows machinery." Again, no references or justification are given.

The most obvious point of these statements is the universal *belief* in the original use of a leather accordion blast, without any evidence for same. Mostly, the reports either do not justify a conjecture, or reference an earlier report that does the same. The exception is the reference to Long, who is describing a configuration that existed, if at all, around one and a half centuries before his recounting. Lacking independent evidence, he is merely recounting lore, which is not reliable.

In summary, the historian statements represent a consensus conjecture that the original blast consisted of one or more leather accordion bellows, but this is not supported by evidence. There appears to be no written reasoning behind the conjecture; my best guess is that this represents historical precedent combined with a lack of contrary evidence.

4.2. Subsequent Blasts

Given the assumption of an initial leather accordion blast, a conversion is necessary to get to the current blast, and possibly a series of intermediate steps and/or improvements. As with the initial blast design, current belief about any conversion and intermediate blasts comes from the official reports. Statements concerning these matters must be gathered and evaluated in the same manner as those for the initial blast. This is done in this section.

- Kurjack [Kurjack 1954] on p. 15 states that blowing tubs replaced bellows “at least as early as 1822, and possibly as early as the 1790’s.” No reference is given.
- In his April 1949 monthly report [Kurjack 1949b], on p. 1, he states “It now appears fairly certain that cylindrical or ‘tub’ bellows were used at this furnace (as exemplified by the last one) at least as early as 1800.” He states that the size, whether single or double acting, and whether there was an equalizer, were still uncertain. However, no justification is given.
- In his June 1949 report [Kurjack 1949a], on pp. 1–2 Kurjack makes several statements. The first is “It has been established earlier that blowing cylinders or ‘tubs’ were used at Hopewell Furnace at least as early as 1798.” He gives no reference or justification, and his only earlier remark is for 1800 instead of 1798 (in the April report). He then describes the “earlier type” of cylinders as the Coventry blast, and “crude,” but he provides no references. Finally, he states “It is likely that during the first two decades of the nineteenth century Hopewell Furnace used the crude, single-acting cylinders.” Again, no references or justifications are given. Given the structure of the paragraph and the use of the term “crude,” it seems certain that he means use of the Coventry blast.
- [Apple 1956b] p. II-119 states that Kurjack “believes” that a Coventry blast may have been installed around 1800, referencing [Kurjack 1949a].
- Hugins on p. 10 quotes [Kurjack 1949a and Kurjack 1949b] (pp. 1 and 1–2) as stating that tubs were used “at least as early as 1798”. He also claims an apparent change to a Coventry blast, but references Bining for this. However, Bining makes no mention of a Coventry blast, just general blowing cylinders attributed to Wilkinson.
- R&A on p. 30 claims that the blast “may have been converted from ‘bellows’ to ‘single-action cylinders made of wood.’” Further, “The cylinders moved up and down between four wooden posts, which may have provided support for a protective roof over the blast machinery ...” which is the Coventry blast. He cites Schumacher, which contains no reference to a Coventry blast, nor any mention of archeological evidence for the “four posts.” He also cites [Apple 1956a] pp. 27–29, which simply quotes Kurjack’s June 1949 monthly narrative report. He also cites [Apple 1956b] pp. II-118–II-123, which is just a reference to Kurjack’s claim. He finally cites Hugins on p. 10, already described above.
- Schumacher on p. 7 states that 1801 is the “Earliest indication of the use of blowing tubs, and with them possibly came the east-west wheel pit.” No reference or justification is given.
- In R&A, the footnote on pp. 30–31 contains the statement “Apple believes bellows continued in use at Hopewell until at least 1801, and that double-action blowing tubs were constructed in 1816.” No direct reference is given, but it is presumably [Apple 1956b]. The footnote finishes with “It is not, however, until 1822 that it seems certain that blowing tubs powered the blasts at Hopewell.” No reference or justification is given.
- Donohoe on p. 4 says that around 1805 it is “believed” that the blast was replaced by a 22 ft east-west wheel and “a crude set of casks.”
- [Apple 1956b] p. II-121 states that in 1807 a worker made a cam pattern and did other work associated with the wheel and blast. This was said to suggest a piston rod, but does not state certainty. However, the corresponding KCFO entry is actually “cam (?) pattern ...”, indicating uncertainty even that it was for a cam. No corresponding furnace book entries are found. And in any case, cams were used to depress leather bellows, and so do not imply a cylinder blast.
- Schumacher on p. 22 states that “By 1810, some type of blowing tub, probably not the same as the present tubs, and the east-west wheel were definitely in operation and were constantly in use until 1883.” No reference or justification is given.

- R&A on p. 33 states that in 1818 “It may be that Hopewells blast machinery was changed at this time from the single-action cylinders installed at the turn of the century to double-action pistons inside wooden tubs, called ‘blowing tubs.’ ” No justification is given.
- Yocum on p. 42 states: “Leather bellows that were powered by the water wheel to produce a blast of air to the furnace were also believed by Historian Kurjack to have been replaced by early blowing tubs about this time.” where “this time” appears to be 1805–1810. The reference is to various pages in [Apple 1956b].
- The furnace was out of operation from 1808 to 1816. Walker [Walker 1966] on p. 49 quotes a memorandum: “During which time the works etc. went into decay and after the Decision of the cause Cost near $8000 to repair before the furnace could be of any use.” This is a very high figure and supports any sort of work. However, as described later, this figure is almost certainly wrong.
- Yocum on p. 43 states that in 1816, “Extensive work of an unknown nature on the water wheel and blast equipment is also suggested by the lengthy employment of a millwright.” This is consistent with the general repair work required by the decay, and does not imply new equipment.
- [Apple 1956a] on p. 7 states, referring to around 1805, that a Coventry blast was installed, and later changed to something similar to the current blast. This references [Apple 1956b] pp. II-119–II-120. These pages merely reference Kurjack’s June 1949 report.
- [Apple 1956a] on p. 28 states that is is “likely” that a Coventry blast was installed “by 1805,” with no reason given.
- [Apple 1956a] on pp. 28–29 states: “Perhaps by 1818, the modification of the casks to double action pistons inside wooden tubs had occurred.” He references [Apple 1956b] pp. II-119–II-123. However, these pages are just a summary of data and references to Kurjack’s conclusions, with no justification for the 1818 date of a conversion.
- [Apple 1956b] on pp. II-121–II-122 states that “It is not until 1822, that we can be reasonably certain that blowing tubs similar to the existing (restored) ones, were in use.” His basis is the Dotterer receipt for the piston springs, described later.
- Hugins on pp. 10–11 claims that a modern style blast was installed 1817–1822. He states that this blast was developed in the early 1800s and adopted generally, referencing Bining. However, Bining makes no such detailed claim.
- Gordon on p. 105 states the Hopewell installed blowing tubs *in* 1822, however, this date likely came from Hopewell itself [Gordon 2016].
- The Official Handbook (OHB, [Lewis 1983]) on p. 36 states that “Wooden piston tubs with leather valves were substituted for the outmoded and relatively inefficient leather bellows.” The date is implied to be soon after the 1816 restart. No reference is given.
- Yocum on p. 47 states the disagreement as to when conversion was done, adding that archeologist Leland Abel believed the tubs were installed around 1851, along with changes to the wheel. She also states that there is no specific documentation about the installation of the tubs, but their existence is concluded from the 1852 SM 32 entry about repairs to them.
- Yocum on pp. 80–81 describes the restoration of the blast, including the restoration of one tub and the receiver, and the construction of a replacement for the second tub. “A new laminated piston, valves and leathers, and metal springs were installed.” Concerning the receiver, “The interior was re-leathered ...” This implies that the same leather components were used in the period. The same comment applies to “springs.” P. 148 presumes that one tub is the 1879 version, but says which is that one is not known.

- Donohoe on pp. 4–5 says that “lack of evidence” implies that the wheel house was not altered between 1818 and 1879, and that the wheel house was restored to it’s 1879 configuration.
- Cass on pp. 2–3 states: “Historical research has established that ‘tubs’ were used at Hopewell at least as early at 1798” and “This machinery probably consisted of two cylindrical casks, one fitting inside the other, moving up and down between four wooden posts; the air, blown into a leather bag, was then fed to the furnace through an iron pipe.” He is claiming a Coventry blast, although without evidence. Interestingly, he quotes the “bag” instead of the “conduit,” indicating that someone may have been reading White. No reference or justification are given.
- Cass on p. 3 states: “Many improvements gradually led to the development of the so-called double cylinders ...” and “Historical evidence indicates that such a mechanism was installed at Hopewell between 1817 and 1822” with a larger receiver in 1881. No reference or justification are given.
- Kurjack on p. 35 states that after a “rebuild” in 1828, “its top capacity, because of improved blast equipment, increased to 1,000 tons or more.” No reference or justification are given.
- Walker appears to make no mention of the blast, except for a single reference to “blast bellows” on p. 20.
- Cass on p. 4 states that the furnace closed in 1883, and that “the water wheel and blast machinery remained undisturbed” for about 50 years. They go on to relate that it was donated to the Franklin Institute in 1930, and disassembled and put under a temporary roof in 1935. It was acquired by the federal government in 1941 and drawings made. This is a credible chain of custody from 1883 to the present, so we can be reasonably certain that we know the details of the 1883 blast.

This set of statements is similar to those about the initial blast in that they fail to provide justifications, or they reference earlier conjectures that do the same. Without statements of justification for the conjectures, the report statements have limited utility. In general, conclusions will need to be drawn from basic data and not these statements.

5. Water Wheels and Headraces

An understanding of changes to the water wheel and headraces is desired both for its own sake and for how such changes might inform changes to the blast. The wheel can relate to the blast by physical configuration, and the headraces and wheel collectively relate to the blast through the amount of power that can be provided. Of particular importance is a claim that the west headrace conversion (described below) resulted in such a reduction in power that a new wheel and blast had to be built. (This claim appears to not be justified.) This section reviews statements and evidence concerning these components, and presents analysis concerning possible changes over time and their relationship to the blast. The main relevant conclusion concerning blast evolution is that a change in the blast, driven by other factors, probably drove a change in the wheel configuration.

More specifically, the following issues are examined:

- The number and nature of different wheel and headrace configurations.
- The diameter of the original water wheel.
- The elevations of the earlier headraces.
- The timing of wheel and headrace conversions.
- The plausibility of a significant power loss with the west headrace conversion.
- The relationship between changes to the blast and changes to the wheel or headraces.

5.1. Configurations

The current water power configuration at Hopewell consists of an east-west oriented wheel fed by a headrace to the west, to the dam that creates Hopewell Lake. The wheel was formerly also fed by the east headrace, which is partially extent but not in use, although said to be restorable. This is taken to be the configuration at the end of the historical period based on a lack of evidence of changes from then to the acquisition at the start of the modern period.

Archeological evidence demonstrates the existence of a former north-south oriented wheel pit, and a former "old" west headrace, much longer than the current one and not associated with the dam. There is also interview evidence concerning the old west headrace and the conflicts that lead to its abandonment. The complete lack of evidence for additional wheel pits and races, including there being no known claims for same, leads to the conclusion that, over the total life of the site since ca. 1771, there were exactly two wheel/pit configurations, one east headrace and two west headraces, the newer one being the current one. Lack of claims and evidence to the contrary leads to the assumption that each of the two wheel pits contained a fixed size wheel, although the two types were probably different sizes, and were replaced when they rotted.

By necessity, there was a conversion of the wheel to the current pit, a conversion (relocation) of the west headrace, and construction of the dam. Some historians believe that these conversions went along with changes to the blast, and hence that blast changes might be dated by wheel/race conversions. More generally, there are claims of causal relationships between various events.

5.2. Statements from Hopewell Reports

This is a summary of wheel and race related statements from Hopewell reports:

- Cass on p. 1 states: "Tradition has it that the first Hopewell Furnace water wheel was a 30-foot overshot wheel running north and south ..." partially verified when archeological investigation uncovered a north-south wheel pit and forebay pier located north of the present wheel pit. No references are given beyond the mention of the archeological work, and no evidence is cited.
- [Long 1930] on p. 6 states that "For many years after the furnace was built the water was brought in open ditches or long head races. The one that came in from the west was nearly two miles long, and the one that came in from the east was fully one mile long. This water flowed on an overshot wheel fully thirty feet high."
- The NPS List of Classified Structures database indicates an east head race length of $1\frac{1}{5}$ miles.
- [Apple 1956b] on p. II-86 states that Harker Long is the authority for the 30 ft diameter of the original wheel.
- Schumacher on p. 2 states that the north-south wheel pit is now filled in, and was "used prior to 1800." No justification for the date is given. On p. 3 the iron pipe that was part of the east headrace is mentioned.
- Figure 16 in [Catts 2002] reproduces an archeological drawing by Cotter for the 1950 Archeological Operation 1. This shows the east headrace buried pipe passing south of the charcoal house. It is marked as a "6 inch pipe" and has one elevation, 494.5 ft, marked as "top of pipe." This is confirmed by text and drawings in [Cotter 1950b].
- [Appleman 1936] on p. 31 states "No evidence remains at the site of the water wheel pit to show how the water from the garden head race came over the wheel." This is stating a lack of east head race remains at it's west end.

- Appleman further states that the east head race continued in a pipe under the ore pile, and then in a wood trough 1 ft wide by 6 in high, carrying the water at a right angle to the wheel, to a spout that turned the water down onto the wheel in overshot fashion. He is almost certainly quoting Long.
- Appleman continues, again almost certainly quoting Long, that the original wheel was 30 ft in diameter, and "Since water came in at a 30 foot level above the bottom of the wheel pit, it dropped 6 or 8 feet before striking the wheel." and "However, the amount of water that went over the wheel from the east race was comparatively neglible, [sic] the head race from the dam supplying practically all the power for the breast wheel."
- [Long 1930] on p. 6 states that "... the Company built a dam close to the furnace, but could only obtain a head of 16 feet, whereas they had a head of 30 feet before, which gave them more than double the power they had after they built the dam."
- Schumacher on p. 22 states that "There seems to have been a great loss of power when they changed over from the north-south wheel to the breast wheel." No computations or other justification are shown, nor any references given.
- Schumacher on p. 25 states that excavation found the bottom of the north-south wheel pit, which was clay, at an elevation of 469 ft, and that the elevation of the bottom of the current wheel pit is 467 ft.
- On p. 26 of [Appleman 1935], he relates following portions of the east and old west headraces. He states the east race to be about one mile long and the old west race to be over one mile long. Schumacher on p. 21 relates this, but states that Appleman only found parts "several miles" from the village; Appleman's own statement is presumably more accurate.
- Hugins on p. 9 states that initially there were two headraces, the old west race and the east race, each over a mile in length. He references [Appleman 1935] pp. 11 and 26, above.
- Hugins on p. 9 also states that a new wheel was installed between 1790 and 1810, 22 ft in diameter with an east-west orientation. He claims that this led to the damming of French Creek $\frac{1}{4}$ mile west of furnace. He references [Appleman 1936] p. 31, above, which is not backing up these statements.
- Cass on pp. 1–2 states that the present wheel pit "was first used, according to tradition, and not contradicted by historical evidence, in the 1790's, when a 22-foot breast wheel was constructed, running east and west." No evidence is cited.
- Schumacher on p. 4 states that the new west headrace "was put into use probably around 1800." No evidence is cited.
- Kemper (as always, referring to Long's statements) states that prior to 1800 the wheel was a 30 ft overshot type; the Hopewell Lake dam was constructed between 1805 and 1810; the new (current) west head race was 16 ft "lower in elevation than the East or West races had been ...", which made it "necessary" to install the 22 ft breast wheel; and that the east head race was not lowered.
- Schumacher on p. 21 states that "This new west head race was some 16 feet lower than the old west head race or the east head race had been, and it was necessary to install a 22-foot breast wheel." No evidence is cited.
- Kemper has Long stating that the west head race dispute was said to be in 1805, with Warwick furnace, and that the race was abandoned. Also that the east head race alone was insufficient to run the wheel, and that the dam was built between 1805 and 1810.

- Cass on p. 3 states that "tradition" holds that the original west headrace was abandoned between 1805 and 1810, but he believes that abandonment was "a decade or so" earlier, and that this lead to the new (present) 22 ft wheel. No evidence is cited.

- [Apple 1956a] on p. 7 states that "About 1805, during the life of the first Bridge House, the first wheel pit and wheel were abandoned and a new wheel pit and wheel were built whose long axes were approximately perpendicular to the center line of the bridge-ramp combination." This references [Apple 1956b] pp. II-85–II-86, but those pages clearly state only that "it is believed."

- [Apple 1956a] on p. 28 states that "About 1805, the water wheel was changed in location so as to operate in an east-west direction." No reference is given.

- On [Apple 1956b] p. II-118 is a statement that the dam broke on 29 April 1807, referenced to the account book for 1802–1811 at the Berks County Historical Society. However, no such book exists there, but there is a journal for 1802–1804, casting doubt upon the existence of a journal for an overlapping range. There are no such entries in SM 4, the main journal for the time period. He then states that "We are reasonably certain the Hopewell dam and the existing east-west water wheel pit were erected about the same time (see North-South Wheel Pit, C3)." The "North-South Wheel Pit" merely refers to the report section starting on p. II-85 that describes this wheel pit; there is no evidence quoted for the construction date of the new east-west pit. Apple is stating that the dam implies that the east-west wheel was already in place, but gives no evidence. A historian notecard has entries for dam damage on 29 April, 27 July and 14 September 1807, but no references.

- Yocum on pp. 41–42 restates the above Apple claims, but adds "Alterations to the water wheel and the old west head race at Hopewell Furnace are said to have been made around 1805-10, although no firm documentation has yet been found for this change." and "The assumption that the orientation of the water wheel was changed at the same time as the building of the Hopewell dam is purely speculative, however."

- Hopewell Cultural Resource Manager Becky Ross states that there were also dams on the east headrace, so caution is required in assuming which dams are referred to.

- On [Apple 1956b] pp. II-134–II-135, two claims are made as to the construction of the Hopewell Lake dam and new west headrace. One places this at 1805 based on a dispute with Warwick Furnace, referenced to a Harker Long interview. The second is for ca. 1800 due to a dispute over water rights and is based on an interview with the Smith brothers. This appears to be the end of the line for evidence for the date of the Hopewell Lake Dam: there is nothing except interviews conducted almost a century and a half after the event is said to have taken place.

- Yocum on p. 38 essentially restates the previous statement, but accepts the Warwick option, and references [Apple 1956b] p. II-134.

- By Kemper, Long claims that the east head race went into a wood gutter at the Big House gardens, thence to the furnace, except that it was in a "metal pipe" under the ore pile. (The ore pile was at the south-east corner of the charcoal house, over the presumed line of the current cast iron pipe.) No time is given for this.

- [Apple 1956b] on p. II-8, referring to east race and it's pipe, states that "Where the underground pipe broke out of the furnace bank, the water probably entered a wooden trough through which it was conducted to the water wheel (Building 82). Long said that this trough, which was one foot wide by six inches high, ended so that the water dropped six or eight feet on the wheel as an overshot system. Long indicated that the water was dropped from about 30 feet above the wheel pit floor, at approximately the height needed to turn the former water wheel of 30 feet diameter (see North-South wheel, C3). The trough brought the water to the center of the 22 foot diameter water wheel, and the water was turned down onto the wheel by a spout."

- [Apple 1956b] on p. II-9 states that in an interview, Thomas Hoffman claimed that east and west races joined before the wheel.
- On [Apple 1956b] p. II-9, an interview with Charles Care has him stating that there were two separate headraces, with the west race water striking "a little below the center" of the wheel, and the east race water "dropped on the top of the wheel". Similarly, on p. II-119, Apple states that "The water wheel remembered by Long and others was a combination breast and overshot." Note that both statements can apply to either wheel.
- Cass on p. 2 states, in reference to the above 22 ft wheel, that "No evidence has been found of any important structural changes in the basic wheel pattern during the reminder of the operating history of Hopewell Furnace, although the mechanism had to be continually repaired and was several times partially or completely rebuilt (roughly every fifteen years)." This is a claim of a constant wheel arrangement for the entire historical period starting from when the east-west wheel was installed.
- Kemper states that the water wheel was replaced by one of the same size in the fall of 1879, while Long was manager; this is presumably a statement by Long.

No entries have been found in the account books that indicate conversion of either the wheel or the west head race. There are numerous entries concerning repair of the wheel, building a new wheel, and work at the races. However, all of these can apply equally well to any wheel or race. As described, there are claimed to be entries about damage to "the dam," but there were apparently dams on the east headrace in addition to the new west headrace. Thus there is no hard evidence about past structures except that uncovered through archeology.

5.3. Elevations

Elevations involving the wheel, races and surrounding ground are essential for evaluating claims of power and changes. The basic elevations were established by Cotter during archeological work in 1950, and available from the drawing reproduced in Catts. These concern mainly ground and wall tops for surface and buried features. The water wheel is tied into this in sheet 3 of [Higgins 1949], which is drawing set 2062A. These are plans for the water wheel and have certain Cotter elevations marked on them. Finally, various steel tape measurements were taken in 2015 and optical level and rod measurements were done in 2016. The latter are described in an unpublished "Note on Leveling for the HOFU Furnace Group" by this author.

The basic elevation for the current wheel is set by drawing set 2062A, which marks the elevation of the top of the south main (concrete) sill as 476.86 ft. This also assigns an elevation to the axle center of 479.11 ft, placing it 27 in above the south sill top. The center on the other side was measured to be $25\frac{1}{2}$ in above the north sill by steel tape. However, the two sills have about the same elevation. This latter measurement was taken while the wheel was operating, and is deemed less reliable than the first. The axle elevation is taken as about 479 ft. For a diameter of 22 ft, this places the wheel bottom at 468 ft and the top at 490 ft.

Steel tape measurements place the bottom of the current flume at 9 ft 8 in above the top of the north sill. Assuming an inch for flume bottom thickness, the elevation of the bottom of the water is taken as 486.7 ft, giving a theoretical head of 18.7 ft. Recall that Schumacher on p. 25 states that the elevation of the bottom of the current wheel pit was 467 ft during the 1950 dig, and the old pit bottom was at 469 ft.

The east head race consists mostly of a $1\frac{1}{5}$ mile long ditch that is substantially extent and said to be original to the furnace construction. The final approximately 200 feet are in an underground pipe through the furnace area, presumably to keep the race out of the way of operations. The installation date of the pipe is important but unknown, and partially the subject of this section. The ditch and pipe join at a grate with an elevation of 500 ft. The south (lower) berm of the ditch in this area is at 501.8 ft. These two elevations are from 2016. The grate elevation probably reflects the operating period, while the berm elevation is less certain.

By the Cotter work, the top of the pipe, under the connecting shed about 50 ft from the charcoal house, has an elevation of 494.5 ft. It is called "6 inch pipe," placing the invert at about 494 ft. This is a 6 ft drop from the grate over about 150 ft, about $\frac{1}{2}$ inch per foot.

The pipe continues toward the old and current wheel pits, exiting a retaining wall close to 5 ft below the wall top, around 34 ft from the previous Cotter measuring point. The invert elevation is about 493 ft at the wall.

The fact that the old wheel pit bottom was 2 ft higher than that of the new/current one is taken to mean that the bottom of the original wheel was 2 ft higher than the current one, or at 470 ft. The top of the original wheel was then higher than this by it's diameter, or at 500 ft for the claimed 30 ft original wheel.

5.4. The Current and "New" West Headraces

It appears well established that the current headrace is the same as the "new" west headrace, and in particular, that the elevation of the latter is known by measuring the former. On p. 2 of [Cotter 1950b] archeologist Cotter states that "The reconstruction of the West Head Race is a simple matter of cleaning out a well-defined old ditch, repairing the retaining bank in places and elevating the flow at the east end on a tressel, based on the extant stone piers, ending at the water wheel." On p. II-136 Apple states that "The restored forebay is approximately two feet wide and two feet high, on stone piers, some of them original, that are approximately four feet wide." (The forebay is the wood flume or trough that directly feeds the wheel.) This gives high confidence that the current headrace arrives at the wheel at the same elevation as during the historical period.

5.5. The Old West Headrace

The existence of this a ways from the furnace is credibly established by it having been followed by Appleman. However, there is no claimed evidence for it close to the furnace, so it's route and elevation profile are unknown. The best we have are interview statements about it's elevation *relative* to the east head race or the original wheel. Note that there is no evidence for the wheel pit having been moved after 1800, and no claims of a conversion after the early 1800s, so the interviews are recounting events at least a century and a quarter old, and constitute lore.

In the statements enumerated above, there are interview claims about the relationship between the east and old west races. There is one claim that the races joined before the wheel, which puts them at the same elevation. Other claims are that the west race was lower. There are no claims that the old west race was elevated above the ground or the east race. While the lore is uncertain, all the lore that exists is that the old west race was no higher than the east race; there is none for the opposite.

Also note Long's claim that the east head race turned the original wheel in an overshot fashion. Even with little confidence in Long's recounted lore, the higher-lower relationship between race and wheel inspires more confidence than the numbers. If the east race came in above the original wheel, it is unlikely that the old west race was higher, as this would serve no purpose. Again, the assumption is that the old west race was no higher than the east race.

Note the above Schumacher claim that the old west headrace was 16 ft above the known new west headrace. As described with the east headrace, this appears impossible and is dismissed.

Overall, it is taken that the old west headrace terminated at the wheel at an elevation no higher than that of the east headrace.

5.6. The East Headrace

While there is little question concerning most of the east headrace (EHR), the most important part is unknown: the elevations where it terminated at the wheels. Most statements by Long clearly represent the current wheel, since the water is “dropping” a distance onto the wheel. However, the details cannot all be correct. There is no archeological evidence for the final approach to the wheel pits. So constraints must be estimated from other available evidence.

The east headrace is taken to be well known up to the point where the ditch terminates at the entrance to the pipe just west of the Big House and garden. No evidence is claimed that this was altered during the operation of the furnace, or during the period thereafter, including during the restoration. This places the pipe entrance elevation at 500 ft.

The race is further well known from some unknown start time during the operation to the present by the pipe, which was dug up by the archeologists. There is no evidence of this pipe being placed in the historical period. However, as elaborated below, it appears impossible for it to be original to the furnace. This makes the original elevation profile uncertain. Further, it ended well short of either wheel pit, so it’s final elevation at the wheel(s) is unknown.

Note that Long claimed (via Kemper) that the east head race was in a wood gutter from the gardens, except for a pipe under the ore pile. This is dismissed. As described below, the entire pipe run is likely from before 1850 and possibly pre-1800, so Long is describing events from before he came. Also, the race crossed a road between the gardens and ore pile, so it would have made little sense to have trenched in a gutter instead of just burying a longer pipe.

The absolute maximum terminal elevation is taken to be 500 ft, being the same as the last reliably known original elevation, that of the pipe entrance at the end of the ditch. This is taken as unlikely, since the ditch is said to have a slope, implying that the remainder of the conveyance probably had one. So the practical maximum is some unknown margin below 500 ft. Plus, there is good reason to have a slope to aid in scouring the conveyance. Note that this assumes a water conveyance other than the current pipe, which is much lower than 500 ft.

The minimum likely elevation is taken from the existing pipe exit at the retaining wall. The invert measures at an elevation of 493 ft. This might be around 20 ft from its termination. Extending the pipe at the average pipe slope of $\frac{1}{2}$ inch per foot would place the terminal elevation at around 492 ft. Note that the final section of the pipe is a modern installation. An alternative is to estimate from the Cotter elevation under the connecting shed, perhaps 55 ft from a wheel. The invert is at 494 ft. Taking the same average slope from here results in a terminal elevation of about $491\frac{1}{2}$ ft. Using the modern DWV slope of $\frac{1}{8}$ inch per foot gives $493\frac{1}{2}$ ft.

To elaborate on the modern origin of the final pipe section (including the wall penetration), [Appleman 1936] on p. 31 states that nothing remained at this end. Photo 106-46 from [Cotter 1950a], “West end of East Head Race pipe,” is a photo of the uncovered west end of the pipe, within the excavation area. The Cotter diagram is also clearly showing the excavation area slightly extended to the west in order to discover the end of the pipe. On p. 27 of [Schumacher 1956] we have: “The East Head Race pipe was entirely uncovered, flushed out, repaired where broken, *added to* and then covered again, with a grate placed at its eastern end where the water from the open ditch enters the pipe.” (Emphasis added.) So the 1950s workers clearly restored the pipe, and apparently relaid the west end. No claim was made that this final part followed archeological evidence. Hence, the final western profile and wall penetration cannot be assumed to reflect that of the operating period.

A critical question is what the EHR terminal elevation originally was, ca. 1771. As stated above, the west end of the pipe may not have followed the operating profile, but this is not known. The larger question is the overall pipe profile. More specific is whether the current pipe profile served the original wheel, which is not the same as being original itself, as it could have been installed sometime after the original wheel but before the wheel conversion. Note that for anything but original installation, the pipe may be masking the original conveyance of the EHR water to the wheel, leaving us with no evidence until the ditch/pipe junction.

The first issue is whether a cast iron (CI) pipe installation was even possible ca. 1771. Use of CI pipe prior to 1800 is possible, although it would represent early adoption, and installation in the colonies in 1771 possibly would have been unprecedented. By [CISPI 2006], CI pipe was first used in 1562, with the first "full-scale system" in 1664, both in Europe. The bell and spigot joint was invented in 1785. Pipe production in the US was said to start in 1819. However, pipe was imported prior to this from England and Scotland, and Philadelphia was installing it for water distribution starting in 1804 [Schladweiler 2016].

P. 27 of [Schumacher 1956] describes sending "A cracked 9-foot section of 6-inch pipe, complete with bell, ..." out for analysis. The result was "From our investigation and tests on this pipe, we feel it was produced prior to 1850. The parting line on the pipe showed that it was cast on the side, which was common practice in early pipe." and "Because of the high phosphorous, we are also of the opinion that it is old pipe." No identification marks were found. This puts a ceiling on the pipe age. A floor is established by the use of bell and spigot joints. The Cotter diagram is indicating a bell and spigot junction system. Photos in [Cotter 1950a] clearly show a bell and spigot system, especially photos 106-8 and 106-12. And the above quote from p. 27 includes "complete with bell". This is indicating installation after 1785. It is therefore almost certain that the existing pipe is not the original EHR conveyance through this area.

However, even if the current pipe is not original, serving the original wheel is possible, but the time window is narrow. As described later in this section, many of the claims for wheel conversion are dismissed in favor of prior to 1800, with light circumstantial evidence for around 1785. The window opens in 1785 with the development of of the bell and spigot joint, and lasts for from a small number of years to a decade or so. Note that this timing also makes plausible the idea that the current pipe was installed as part of the wheel conversion, again, masking the original conveyance evidence.

Supporting an early installation is lack of any evidence of installation after 1800. There is no mention of the pipe installation or modification in any report. None of the historian reports, nor the more modern summary reports, makes any claim of an event; the only description is "how it was." My own survey of all relevant account books in existence produced no entry that clearly related to the pipe system. (The books do not cover all time, so it is possible that work was described in a missing book.) The only pre-1850 work that might be associated with the pipe involves two entries for work at the "trunk." As described later, "trunk" appears to involve a head race, so it could be the pipe, or the west head race flume. In both cases the work concerns a "new trunk," which implies replacing something already present. But there is no evidence as to what exactly was done. This adds support to a pre-1800 installation, where there is almost no documentation.

Note that on p. 21 of [Schumacher 1951] is the claim "This new west head race was some 16 feet lower than the old west head race or the east head race had been, ..." Since the new west headrace has a known elevation, this would seem to place the east and old west headraces. However, he gives no reference or justification, and Long is a clear possibility. Further, the new west race is at 486.7 ft. This claim places the EHR elevation at about 503 ft. This is 3 ft above the bottom of the current ditch and over 1 ft higher than the south berm of the ditch near the pipe entrance. For both reasons, the claim is dismissed.

The ground level may or may not give evidence of the head race elevation. The existing invert is at about 494 ft under the connecting shed. The 2016 ground is at 499 ft and the 1950 ground was at 498 ft, placing the pipe 4-5 ft underground in modern times. Given that it starts at 500 ft, there is little reason to bury it so deep. However, by the excavation profile in [Cotter 1950b], the elevation of undisturbed soil at this point was about 491 ft, and the pipe was shallowly buried in rock fill on top of same. So the pipe was barely dug in, and possibly not at all, possibly covered after having been laid on top of a partial fill. So it appears that any plausible EHR elevation was possible without significant digging. If the race had been sitting on the original ground (taking this as the undisturbed soil line), it would have been at elevation 491 ft about $\frac{3}{4}$ of the way from the ditch to the wheel. Assuming another 1 ft drop to the wheel (reasonable but less steep than the average to this point), gives a terminal elevation of 490 ft.

The original grade is also consistent with any race elevation up to 500 ft. While such a race would appear to impede the path to the stack, it would only increase the slope, without creating a hump. The ground elevation at the charcoal house door in 1950 was 497 ft. The ground on the interior was not likely to be lower due to the problem of lifting the charcoal out, and it is not likely that the charcoal house had been partly filled in, as they needed all the volume they could get. (The wall foot elevation is apparently unknown, as there appears to have never been a dig at the charcoal house.) The stack top is at 503 ft by the 2016 leveling. Assuming the race could be in a trough with 6 in high sides, a plank ramp might plausibly cross the race at 501 ft. If run on a straight line (following the current pipe) this would be a 17% slope from the old charcoal house south extension, which seems undesirable. However, if the race were routed south to pass under the ramp at Wall A, the slope would be reduced to 5%, the same as for the current bridge.

Additional but unclear data is on p. 25 of Schumacher, referring to the area just north of the old north-south wheel pit: "The masonry of this area, which I believe was the forebay of the Old Wheel Pit, rested upon a footing of yellow sterile clay, at elevation 482.5." Taking "forebay" to mean a reservoir places this at a very low elevation. However, on pp. 30–31, "forebay" is clearly used to refer to the west head race's flume, which is not a reservoir. So what exactly Schumacher meant by "forebay" is unclear. Having water enter at this elevation implies an effective head of less than 10 ft, which seems very unlikely. He probably takes this level as the base of a support for the flume, which could place the terminal elevation anywhere.

To summarize the major evidence for the original configuration of the east head race pipe and any other earlier conveyance:

- The pipe intake elevation constrains the maximum EHR terminal elevation to be some unknown margin less than 500 ft.
- The current pipe elevation profile and original ground elevation plausibly support an EHR terminal elevation as low as 490 ft.
- The bell and spigot joints place the installation of the current pipe after 1785, so it is not original to the furnace construction, but could still have served the original wheel.
- It is quite plausible that the current pipe was configured for the current wheel, although the evidence is only circumstantial, i.e., the good fit. This raises the possibility that the current pipe configuration is masking the original conveyance of the EHR water through the area.

5.7. Size of the Original Wheel

The two main points to start with are that there is no support for the claim of an original 30 ft diameter wheel, and that the real constraints come from the terminal elevation of the EHR, since the wheel bottom elevation is presumed known at 470 ft from the wheel pit bottom archeological evidence.

The most basic problem with the 30 ft claim is that it almost certainly comes from Long, with no claims of any other source. Apple directly states that Long is the authority, a direct claim that no evidence exists. While 30 ft is repeated often, no justifications are given. As stated before, Long is describing affairs close to a century and a half before the recounting, and apparently merely repeating from an old scrap book whose nature is unknown. There is no credibility to this.

There is a further problem with the claim that the water "... dropped six or eight feet on the wheel as an overshot system. Long indicated that the water was dropped from about 30 feet above the wheel pit floor, at approximately the height needed to turn the former water wheel of 30 feet diameter ..." There is an internal problem with this in that the wheel bottom cannot rest on the pit floor, so an overshot stream cannot be 30 ft above the pit floor for a 30 ft wheel. The bottom gap should be at least over 1 ft. More importantly, none of the statements imply that Long claimed to be describing an original headrace, rather than the race as he knew it while he was there. As described below, this is the east headrace whose pipe invert is at an elevation of 494 ft at a distance of over 40 ft from the current wheel, whose top is at 490 ft. This indicates a drop of less than 4 ft, not the 6–8 ft claimed. This not only casts doubt on the details of Long's memory, but represents a conflation of the current and original wheel/race configurations that is internally inconsistent. This casts doubt on the 30 ft wheel claim, and indicates that almost nothing is known about the original wheel.

Another problem is on p. II-8 of [Apple 1956b]: "Where the underground pipe broke out of the furnace bank, ..." This is a problem in that the existing ground around this point is less than 500 ft, while the top of the stone wall at the penetration is about 498 ft. If a pipe is to "break out" of the bank, this implies that the wall is non-trivially higher than the pipe. For the 30 ft wheel, the pipe would have to be above 500 ft, implying a wall considerably higher than exists, and a very different slope for the ground and bridge. So the overall description is suspect.

There is some relevant archeological data in [Schumacher 1951]. As shown on the diagram "Walls E, A, G South Face Profile through line $21\frac{1}{2}$," there are what appear to be wheel support structures in the old wheel pit. The "Old Wheel Pit West Wall" has a top at elevation 479.3 ft, while the opposite "Shelf" is as 479.1 ft.

It we take the practical minimum bearing and axle support as adding 1 ft in height, for the axle center resting on this shelf, the wheel diameter for a bottom at 470 ft would be 20 ft. This is taken as the practical minimum wheel diameter, assuming they intended that the wheel fit the pit. The axle supports can be higher, of course. Taking the existing wheel structure as a guide, the axle center is around 4 ft above the stone, for a 26 ft wheel. Higher still could support a conceptual 30 ft wheel. The diagram is showing what is probably the pit width to be about 8 ft, which is the same as the current pit. This makes it reasonable to assume that the original wheel had about the same width as the current one.

It is important to point out that a 20 ft wheel is not only plausible, but fits the data well, despite being at odds with the lore. A 20 ft wheel would be an overshot type, presumably by both headraces. The power estimates (below) assume a 14 ft effective head on the current 22 ft breast wheel, due to lack of an apron. Assuming a similar percentage, but acknowledging no knowledge of the original wheel design, indicates a 15 foot effective head for a 20 ft overshot wheel. This would explain the lowered wheel pit for the new wheel, that it permits the new west head race to provide the same head as the old one, while using a larger diameter to take advantage of the elevation of the east headrace.

In summary, the archeological evidence is for a wheel whose bucket width is around 4 ft and whose diameter is at least 20 ft. Given a wheel bottom at 470 ft, the terminal headrace elevation range of 490 ft to under 500 ft gives a diameter of 20 ft to under 30 ft.

5.8. Power Loss from the New West Headrace

A significant claim, and a possibility, is that the west headrace conversion and resulting loss of head caused an unacceptable power loss that lead to the design of a new wheel and pit, and possibly a new blast. A loss of head can be compensated for by an increase in flow, which presumably is available by definition since they went to the trouble of making a new head race. If the original wheel buckets were large enough, no alteration was required; otherwise a new wheel with larger buckets was needed. Sufficient information does not exist to prove or disprove the claimed need for a new wheel. However, power and water flow computations provide an estimate of the effect of the race conversion. This indicates that the change was likely manageable with the original wheel.

Also, since the old wheel pit appears to have the same width as the current one, it is likely that a power loss due to a narrow wheel could have been solved merely by dropping in a wider one, and not requiring any change in the pit. A stronger statement is that the conversion was likely manageable without converting to the new wheel pit.

As an aside, Schumacher declared that there "seems to have been a great loss of power" *with* the change to the new wheel, but gives no justification. This is extremely odd, since if they went to the trouble of building a new wheel and pit, and lowering the pit floor, it is very unlikely that they would have settled for less power than they needed. This claim is dismissed.

Another basic question is why a power loss would have lead to a new blast. A new blast will only help if it is significantly more efficient, for the same discharge, as the old one. For example, converting from a single acting cylinder blast to a half-size double acting one will give approximately the same discharge, but still require approximately the same power; nothing is gained on a power loss. A leather accordion bellows may be significantly less efficient, requiring more power for the same discharge. Pp. 16–17 of [Sticht 1906] has come comments to this effect, although this is sufficiently lacking in details so that the meaning of the numbers is not clear, and the leather bellows details may be from the 1600s. In any case, the comparison cannot be reasonably done, since we do not know that a leather blast ever existed, and if it did, we do not know the construction details. So a power loss leading to a new blast cannot be ruled out, but there is no evidence for it. And in any case, the above argument is that a new wheel alone would probably have permitted the same power from the new race configuration and original pit, eliminating the need for a more efficient blast.

However, to see whether this argument is plausible, an estimate of the power change with the west headrace conversion needs to be done. The particular question is whether the same power can be obtained with either the original wheel, or a new one that would be a drop-in replacement.

The first question is how much lower the new west head race was than the old. This has no clear answer, since there is no evidence for the old one. As argued above, it seems unlikely that the WHR had a terminal elevation above that of the EHR, so the conservative approach is to assume that the old WHR was at the same level as the EHR.

The new terminal elevation is taken as 487 ft. It the original wheel diameter range is taken as 20–29 ft, the race elevation range is 490–499 ft, and the head loss is 3–12 ft (not the 16 ft claimed). This loss is only partial, since the EHR did not change. How much this matters depends on what fraction of the total flow was in the old WHR. The power lost from a partial decrease in head must be made up by an increase in flow in the new WHR, which can be done *provided* the wheel buckets can hold the increased volume and the new WHR can supply it. It can still be done, assuming sufficient flow, if an adequate new wheel can be dropped in place.

So an estimate of the original flow and allocation between the EHR and old WHR will give an estimate of how much additional flow would have been required to make up the head loss. This can then be examined for plausibility. If it is deemed plausible that the old wheel could hold the new volume, then there would have been no need or incentive to build a new wheel (at least from a power standpoint). If deemed implausible, then there would have been an incentive to build a new wheel with a larger bucket capacity in order to recover the lost power. As long as such a new wheel was a drop-in replacement, there would have been no need for a new pit or blast.

The first step is to estimate the furnace air requirement. Overman on p. 408 states that the air requirement in ft^3/min is estimated as the number of pounds of fuel consumed in 12 hours divided by 5. Taking the canonical Hopewell figure of 15 bushes every half hour, at 20 lb/bu, gives 7,200 lb per 12 hours. This estimates about 1,440 ft^3/min.

Given the air requirement and the presumed operating pressure of the blast, the power requirement can be computed. The required water wheel speed can be computed based on the blast's discharge per stroke. The pistons are 6 ft in diameter with about a $2\frac{1}{2}$ ft stroke, so about 71 ft^3 of air is delivered on each stroke, in each direction. This computed air delivery would normally be derated, per Overman on p. 408, but other Hopewell work indicates that that the commonly used number of bushels may be a significant overstatement, so both factors are ignored and expected to offset each other. Two pistons each delivering two strokes per revolution deliver 285 ft^3 of air per wheel revolution. Thus the wheel needs to turn at about 5 RPM. This figure appears consistent with good wheel design for the current wheel. A waterwheel book [I.C.S. Staff 1907] on p. 35 states that the circumferential velocity of a breast wheel should be 3–6 ft/sec. The wheel circumference is 69 ft, so 5 RPM is 345 ft/min, or 5.8 ft/sec, within the suggested range.

Overman on p. 402 states that the maximum practical pressure from wood cylinders is about $\frac{3}{4}$ PSI, and OHB on p. 82 states that Hopewell used this pressure. This gives an average force against which the pistons operate. This means that the piston is subject to about 108 lb/ft^2, or about 3,000 lb total. Over it's $2\frac{1}{2}$ ft travel, it does about 7,500 ft-lb of work per stroke. There are 4 strokes per revolution, or 30,000 ft-lb/revolution. At 5 RPM, this is 150,000 ft-lb/min, 2,500 ft-lb/sec, or about $4\frac{1}{2}$ HP. Assuming a water wheel efficiency of 0.7, this calls for a computed wheel power of about $6\frac{1}{2}$ HP.

Now this can be translated into water for the current 22 ft wheel. Since the current wheel is a breast type, we ignore the incoming water velocity. The mathematical head is $18\frac{1}{2}$ ft. However, there is no mention of a breast (apron) at Hopewell, and none exists currently, so the water will start escaping the buckets before they are at bottom. Estimate an effective head of 14 ft. To get 2,500 ft-lb/sec at 0.7 efficiency, the input must be about 3,600 ft-lb/sec, and at 14 ft head, about 260 lb/sec. With water at 62.5 lb/ft^3, this is about 4 ft^3/sec of water.

The current wheel has 60 buckets, so at 5 RPM, 300 buckets/min go by, or 5 buckets/sec. The bucket capacity at the horizontal position is about 2 ft^3, based on the drawings on p. 8 in [Higgins 1949]. This 4 ft^3 gets distributed across 5 buckets, so each each bucket gets only 0.8 ft^3, or, is only 40% full. The wheel is arguably over-sized.

This flow can be given a sanity check by computing the necessary flow velocity in a channel. If the original new west race flume is taken to have the same width as the piers, 4 ft, and the water flowing out is 3 in deep, then the cross section is 1 ft^2. This requires a water velocity of 4 ft/sec to get 4 ft^3/sec, which is plausible. If the ditch feeding the flume had a cross section of 4 ft^2, the velocity would have been only 1 ft/sec or 0.7 MPH, not very fast. (This may not be a high flow, but the creek better keep flowing. 4 ft^3/sec is 8 acre-feet/day, so it would drain the 68 acre Hopewell Lake at the rate of about 1.4 in/day. This would drain off almost 4 ft of swimming hole each month.)

Now we attempt to estimate how much water flowed onto an original wheel. Consistent with the current wheel, assume no apron. For the 20 ft wheel, take an effective head of 15 ft, and for the 29 ft wheel, take it as 23 ft. The required flows are simply the original 4 ft^3/sec scaled by the ratio of the heads, which incorporates the the same 0.7 efficiency. The flows are 3.7 and 2.4 ft^3/sec for the 20 and 29 ft wheels respectively.

This is where the diameter of the EHR pipe is useful, as it provides, in theory, a computable bottleneck. As a limiting case, consider open channel flow but with the pipe almost full. Take the pressure slope as the pipe slope, based on a uniform slope from 500 ft to the terminal elevation, 490 or 499 ft, over a nominal 200 ft. Assume the Manning Number for a new cast iron pipe, 0.012. Based on one of the various online free calculators [HAWSEDC 2015], the flow is about 1.36 ft^3/sec for a 20 ft wheel and 0.43 ft^3/sec for a 29 ft wheel, or 1/3 or 1/5 of the requirement respectively. This is unlikely to be the case however, since the pipe intake is toward the bottom of the ditch.

Instead, the pipe was under pressure, so this was pipe flow instead. Take the pipe length to the approximate wheel location as 200 ft and assume Schedule 40 pipe in another free online calculator [TLV 2015]. The pipe entrance grate is 2.1 ft below the current grass level at the end of the ditch. This will be taken as the head, although with the ditch set into a steep slope, it is plausible that the south berm was built up higher during operation. This generates an input pressure of 0.91 PSI.

Taking this as the total pressure drop to a pressure of 0 at the outflow, gets a flow of 23,600 gal/hr, or 0.88 ft^3/sec. The presumed downward slope should increase this, but the older pipe will probably decrease it. The relative magnitude of these factors is not clear. This supplies about a quarter for the 20 ft wheel and about a third for the 29 ft wheel. However, the increased slope for the 20 ft wheel will presumably overcome the resistance, as the pipe flow is unlikely to be less than the open channel flow. So for any wheel, estimate that the EHR may have supplied about a third of the necessary water. (The kinetic energy of the water on a proper overshot wheel is non-zero, but appears insignificant here, and is ignored.)

This Appleman/Long quote (from above) needs to be addressed: "... the amount of water that went over the wheel from the east race was comparatively neglible, [sic] the head race from the dam supplying practically all the power for the breast wheel." This claim is in obvious conflict with my estimate that the EHR supplied about a third of the water, but it is dismissed for failing the smell test. A race system of over a mile of debris-prone open ditch and dam(s) is not going to be maintained for decades for a "negligible" amount of water.

Now let the old WHR be replaced by the new west (current) headrace. The flume level drops for the WHR water only, to an effective head of 12 ft. The estimated WHR water that must have increased flow to compensate for reduced head is $\frac{2}{3}$ of the total flow, 2.48 and 1.60 ft^3/sec for the 20 and 29 ft wheels respectively, and the effective head ratios are 15/12 and 23/12. This gives new WHR flows of 3.1 ft^3/sec in both cases, and total flows of 4.3 and 3.9 ft^3/sec, compared to the original flows of 3.7 and 2.4 ft^3/sec.

The important question is whether the original wheel buckets, designed assuming only 3.7 or 2.4 ft^3/sec, can hold 4.3 or 3.9 ft^3/sec instead, factors of 1.2 and 1.6. Given that the current wheel's buckets seem to have been loaded to only 40% of capacity, and assuming this reflects practice instead of incompetence, this seems likely, since they would then be loaded to around 2/3 capacity. They could have been designed for up to 60% capacity and still hold the increased amount. So it is plausible that the change in the WHR did not cause a significant reduction in blast capability. Hence it is plausible that the WHR conversion did not create an incentive to replace the wheel. Also note that if the west head race were lower than the east, which has been claimed although without evidence, the loss would be less, and hence easier to make good on. Finally, given that a wheel of the current design can take 4 ft^3/sec at only 40% capacity, there would have been no problem designing a new wheel to accept the increased flow without any pit changes.

In summary, it is very likely that any head loss from the WHR conversion could have been made good at best with no modifications and at worst with a redesigned wheel only. This would have required a new WHR flow of 3.1 ft^3/sec, while a flow of 4 ft^3/sec seems reasonable. So there is no link seen where the WHR conversion would have lead to either the new wheel pit or a new blast.

5.9. Power, Discharge and an Earlier Blast

A potential objection to the above analysis is that it is based on the current blast, which was unlikely to have been the current one ca. 1800. As will be described later, this was likely a single-acting cylinder blast. In theory, this does not make a difference, because the power depends on the discharge, which does not change (it is still the same furnace). In practice, if the earlier blast were much less efficient, then the power computed above would underestimate that actually required. Given that the EHR flow is fixed, greater old WHR flow would be implied, requiring additional new WHR flow for makeup. However, the above computation works assuming a bucket capacity of only 40%, and a new WHR flow of around 3 ft^3/sec when at least 4 is deemed reasonable. So while a less efficient blast is a concern, it appears that the additional flow would be available.

5.10. Conversion Timing and Relationships

The existence of the old north-south wheel pit, EHR and old WHR are reliably established by archeology and following, as is that the current headrace is the same as the historical new WHR. It is the timing of the wheel and WHR conversions that are uncertain, as is the claim of a relationship between them. As before, there are conjectures but without justifications. The only references are to the 1930s interviews, which cannot be accepted without evidence.

The claims of construction of the Hopewell Lake dam and west headrace conversion (implied to be parts of the same event) are variously placed at ca. 1800, 1805, and 1805–1810. There is claimed to be evidence of the dam existing in 1807, by it's breaking, but as stated, the referenced source material does not exist, and the appropriate main journal contains no relevant entries, so there is no way to assess the possibility that the dam was on the EHR instead. Conversion of the wheel is claimed to be 1790–1810, with no evidence, only the claim that it should have been related to the WHR conversion.

The 1805–1810 claim can probably be dismissed. There is no claimed source for this except from Long, whose description comes from Kemper. This states that the WHR race dispute was with Warwick furnace in 1805, and that it was abandoned. The implication is that this happened *in* 1805, although this is not certain. The further claim is that the EHR was insufficient to run the wheel, which is accepted per the above flow computations. This lead to the dam being built between 1805 and 1810. First, the furnace went out of blast sometime in 1808 and was not in blast in 1809 or 1810, not being restored until 1816. Given that the shutdown was due to a legal dispute, and that restoration was not done until the matter was resolved, a recollection that the dam may have been built in 1809 or 1810 is probably faulty. Second, the combination of statements is not consistent. The furnace operated with typically varying but sometimes good production rates for 1806–1808. If an 1805 event required a shutdown due to insufficient water, the dam would have been required to have been built immediately, it cannot have been built "sometime." This does not tell us which statement(s) is faulty, but there must be a problem, which casts doubt on the general lore. (Remember that this is ca. 1805, but Long arrived in 1867 and is recounting this in 1936.)

The next claim is that of a causal relationship between the dam construction and the WHR conversion. Although neither has good evidence dating it, and there is no direct evidence of a relationship, the relationship is a good assumption. Neither is of any use without the other, and even if the assumption is wrong, no use could have been made of either until both were completed.

The second issue is the implication of report statements that the wheel conversion and west headrace conversion were done at the same time and were somehow related to each other. No evidence is provided either that they were or were not done at the same time. However, I reject the implication that they *should* have been done together, or *should* have somehow been related. There are rational reasons for doing either one first, without plans for doing the other.

First, consider race to wheel conversion causality. The implication of either of Apple's two claims for the reason of the loss of old west headrace water rights is that the loss may have been abrupt, so it is reasonable that the owners wanted to recover their water power as quickly as possible. There would be no need for a new wheel: just run the new race in wherever it hits. This logic only fails if the reduction in head is so large that the furnace can no longer operate properly. As demonstrated above, it is plausible that the existing wheel was adequate.

Now consider wheel to race causality. Obviously the mere replacement of a wheel never calls for changes to the headrace(s). The question is whether converting to a reoriented wheel of different size calls for a new race. The answer is clearly no when the new race is lower than the old one, and there is no evidence that power requirements increased. If the wheel conversion happened in the prehistoric period, it is possible that it was driven by some other factor that included an increased power requirement. In this case, it is possible that the old race was at it's flow limit, so a new race with greater flow was required. However, there is no evidence for or claim of such a situation. So even though the wheel conversion may have been done along with the WHR conversion, there is no *need* for them to have been done together, and lacking evidence, no reason to believe that they were.

5.11. Relationship Between Wheel and Blast Conversions

The final issue is whether there was a relationship between the wheel conversion and changes to the blast design. In theory this is a two-way question regarding causality. However, the possibility that a wheel change caused a blast change is considered very unlikely. The most significant problem is that there appears to be no good reason to convert the wheel except to accommodate a blast conversion. In addition, if the wheel *was* converted for some other reason, there is no clear reason to build a different blast due only to the new wheel orientation. If the original blast is assumed to have been ground-mounted in the north furnace room, the same blast could have been relocated to the open area west of the furnace and south of the current wheel, maintaining the same relationship between the wheel and blast. So the idea that the wheel conversion caused a change in blast design is discounted.

The remaining question is whether there is a plausible blast conversion that would have required the change in wheel reorientation and/or location. The possible reasons are: a larger blast required a more powerful wheel; a smaller and/or more efficient blast permitted a smaller and hence more economical wheel; the existing blast conflicted with desired structural changes in the furnace area; and a desired new blast could not fit where the old wheel was.

The first reason, a larger blast, seems unlikely, but is not ruled out. They would not have installed a larger blast per se, but would have, for some reason, needed more air. The power analysis indicates that the old wheel could plausibly have generated enough power, on the new race configuration, for the furnace as it is today. (The possibility that the prior blast was inadequate does not matter; if the existing wheel was adequate for a new and larger blast, there would have been no incentive to convert the wheel also.) In theory there could have been a blast expansion to handle a future cupola in addition to the furnace, and this cannot be dismissed, since a cupola was known to exist. However, the only evidence for a cupola appears in SM 7, indicating operation for around two months in early 1817, while the furnace was out of blast. This would eliminate any need for simultaneous furnace and cupola blowing. Further, the power analysis indicates that the furnace requirements and current blast would have spun the current wheel at about the speed limit suggested by one source for efficient breast wheel operation. Since they could likely have increased their wheel power merely by increasing it's width, they would have designed a larger new wheel if they needed more power, which suggests that there was no significant power expansion.

In the other direction, a reduction in air requirements, or a more efficient blast, would not have caused a wheel conversion. The only plausible argument for such a situation is that a significantly smaller air requirement and/or more efficient blast could have worked from a smaller and hence more economical wheel. But the records indicate that a proper wheel incurred no recordable maintenance cost, so there would have been no operating savings by throwing out a good wheel before it rotted. They would have simply waited for the old wheel to rot and then replaced it with a smaller one. This would break any temporal relationship between a wheel conversion and blast modification. Finally, there is no indication that the furnace size was reduced. Hence, the idea that a blast reduction or efficiency increase caused a wheel conversion is dismissed.

The next possibility is that some structural change in the furnace area could have conflicted with the then-existing blast. This appears unlikely on its surface, since it is hard to see a minor change being sufficiently important to justify the design of a new blast and moving the wheel. Wanting to relocate the stack would have been another matter, but there is no evidence that this was done. There is no archeological evidence for or claims of qualifying changes, such as a significant relocation of the bridge ramp or the retaining walls.

This leaves the factor of a new blast design to drive a wheel conversion. (Note that a new blast intended to deliver more air can cause a wheel reorientation and/or relocation, but it is the nature of the new blast layout that drives this, not the increased discharge per se.) There needed to have been some conflict between the new blast and some part of the structure, due to the location and/or orientation of the old wheel.

An obvious explanation for the wheel conversion would have been a conversion from a ground-mounted blast to a blast with over-the-wheel cylinders. This would have raised the height of the wheel/cylinder "package." Since the high-mounted cylinders would almost certainly have been driven by piston rods, the cylinder placement with respect to the wheel would be more constrained than something worked through lever arms. There is/was plenty of room in the north furnace room for a ground-mounted blast, and this location would have been convenient for the original wheel. However, an over-wheel cylinder blast may well have conflicted with the bridge. Schumacher on p. 25 states that the east wall of the current tail race formed part of the east wall of the north-south wheel pit. This would put a cylinder beside the wheel and probably under the bridge. Although the early bridge configuration is not obvious, evidence indicates that a conflict would have been likely.

To start, there is a claim that the early stack was lower than at present, which would have meant a lower bridge (at least in part). However, this is rejected. [Apple 1956a] on p. 4 states "It is believed that the height of Hopewell Furnace until 1828 was several feet lower than its present height." This is based on the belief that the stack was raised during much of 1828 and early 1829. Apple claims 528 man days of work on the furnace stack, but the referenced source is a store ledger, and contains nothing of the sort. In addition, he missed an entire blast period: SM 13 p. 60B for 30 May 1829 contains an entry paying Thomas Care $896.57 for "Blowing Furnace from May 11th 1828 to April 12th 1829". Among other things, the claim required rebuilding the stack in spring 1828 but not rebuilding the bridge house until spring 1829, which would have made filling the furnace difficult to impossible. So the claim of an increased stack height is rejected, and the bridge house is presumed to have had it's current elevation profile, at least at the furnace end.

Then on p. 5 of [Apple 1956a] "We believe that the first Bridge House (1771–1828) was narrower by approximately five feet than later Bridge Houses. This is based on an interpretation of the masonry wall remains of the first water wheel pit and its associated walls." Some explanation is on p. 6, and corresponds to the furnace area sketch reproduced at the end of this report. The north end of the bridge sits on "Wall A," which ended about 5 ft east of the west edge of the stack base. They assume that the bridge width was not cantilevered past the west end of Wall A, which seems a good assumption. (Wall A was later extended with Wall E, supporting a wider bridge.) They go on to conjecture a 7 ft gap between the wheel and the Wall C, without explanation. The diagram on (nominal) p. 36 indicates the wheel pit extending only about 8–9 ft past Wall A, meaning that most of the wheel was within the depth of the north furnace room. The latter is constrained on it's south side by the stack, as is therefore the east wheel pit wall. At the north-west stack corner, there would have been a 5 ft gap from the wheel pit wall to the bridge plus probably a 2 ft gap from the wall to the wheel, for a 7 ft gap. Assuming the wheel was aligned with the stack, a nominal assumption if one assumes a ground-mounted blast, this gap would have been uniform.

It appears that the existing blast would have conflicted, in some manner, with the old bridge even with a 7 ft gap. By sheet 1 of [Higgins 1949], the outside edge of a blowing tub is about $5\frac{1}{2}$ ft from the wheel pit edge. The platform structure protrudes at least $1\frac{1}{2}$ ft beyond that. The existing bridge house west edge approximately coincides with the east tail race wall, which used to be the east wheel pit wall. So an estimated 5 ft narrower bridge house would have had it's edge about 5 ft from the wheel pit wall. Hence, one cylinder of the existing blast would have overlapped the original bridge house by around $\frac{1}{2}$ a foot, and the overall structure by about 2 ft. The question then is whether there would have been a vertical conflict.

The top of the current tub platform is at an elevation of about 493 ft, with the tub tops 5 ft higher, and other components higher than that. A 20 ft wheel and the original higher wheel pit is a wash, placing the tubs from 493–498 ft. A 29 ft wheel places the tubs from 502–507 ft.

The elevation of the bridge and supporting trusses at the time, however, is a problem. (The existing trusses extend about $5\frac{1}{2}$ ft below the bridge.) The top of Wall A at about the stack center is at elevation of 490 ft based on the archeology diagram. The floor of the current bridge at the north wall is actually around 501 ft, the the bridge floor running approximately 501–502 ft up to the stack area. (Note that wall work was done during the restoration.) The question then is what was the configuration ca. 1800. It is possible that the ramp started from the wall top at the 490 ft elevation. However, this would have created a roller coaster between the charcoal house and the tunnel head. The grade would have dipped around eight feet from the house to the start of the bridge, then climbed steeply to the stack. Much more likely, there was a supporting structure on top of the earlier (lower) wall, with an intermediate ramp, resulting in a smooth grade from the charcoal house to the tunnel head. This would approximate the current configuration.

For the smallest original wheel (20 ft), the tub top would have been just below the (current) bridge, and would have conflicted with any supporting trusses underneath. The largest conjectured wheel (29 ft) would have the platform barely clearing the bridge, but the (current) trusses might have constricted access to the rods. All intermediate wheel sizes would have conflicted with the bridge and/or the trusses or other support structure.

Ultimately, it cannot be known whether a new blast could have been built around the old bridge. However, in the likely bridge configuration, the present blast, or a plausible earlier and larger one, would have conflicted with the bridge. In addition, even if the new blast was possible with the old bridge, they may have wanted more room around the blast for repairs, and the bridge trusses may have been in the way. So it appears reasonable, although not proven, that a desired over-wheel single-acting cylinder blast would have conflicted with the then-existing (and presumed narrower) bridge house, leading to a wheel conversion.

Note that the idea of a ground-mounted blast does not imply an original leather bellows blast, and a conversion does not imply a conversion to the current blast. Diagrams of early cylinder blasts sometimes show ground-mounted cylinders instead of the above-wheel cylinders of the current Hopewell blast. In particular, some single-acting cylinder designs used counterweights and lever arms in a fashion similar to leather bellows. This type of blast could have used the same wheel configuration as a leather bellows. Also, single-acting cylinders driven from below are well documented, so a conversion could well have been to single-acting over-wheel cylinders, with a later conversion to double acting cylinders.

5.12. Summary

The conclusions resulting from above reasoning concerning the head races, wheels and conversion relationships, are summarized as follows:

- The current headrace is the same as the former "new" west headrace, and hence they have the same elevation profiles.
- The original wheel diameter is not known, but was very likely at least 20 ft and up to less than 30 ft; it was almost certainly not 30 ft.
- The old west headrace was likely at an elevation no higher than that of the east headrace.
- The conversion of the west headrace probably resulted in a loss of head, but probably not a loss of wheel power.
- There was likely no necessary relationship between the wheel and west headrace conversions.

- There is no clear date for the wheel conversion, but it was almost certainly not 1800–1815, due to the ongoing wheel repairs and the furnace shutdown.
- There is no clear date for the west headrace conversion. Due to the concluded lack of relationship with the wheel conversion, this is not very important, so accepting "the early 1800s" is probably good enough, noting that this mostly rules out a simultaneous wheel conversion.
- The wheel conversion required a good reason, and an appropriate blast conversion seems the only likely one. Evidence on millwright work, presented later, places this at either 1816 or else in prehistory. Evidence on the blast, presented later, discounts 1816 and hence implies a wheel conversion in the prehistory period. The wheel repair data indicates an old and/or poorly built wheel by ca. 1800, so the conversion might be before ca. 1790, although with light evidence.

6. Furnace Work Records

This investigation is primarily based on data about work on the furnace and related equipment. This is primarily from entries in the furnace books detailing payments made for work and/or related materials. (Routine hearth and inwall replacement and repair are not included, as they do not relate to the blast.) Similar work was done by past historians based on Kurjack's data, also extracted from furnace books, and mostly depicted on KCFO. The majority of the KCFO entries were also found in the books, and additional book entries were discovered, and a few KCFO entries were not found.

The following list describes all the entries found in either the furnace books or the "Bellows" entries from KCFO. KCFO has an explicit minor column for "dressing." In some cases there is explicit note for dressing bellows. In other cases there is simply a pound/dollar entry in the "dressing" column and the name of the worker in the remarks column.

- 1784: On 30 May Jacob Miller was paid £7.10.0 for dressing the bellows (SM 41 p. 50).
- 1785: On 28 January Mark Bird paid Wm White £2.2.1$\frac{1}{2}$ for 5$\frac{3}{4}$ days of work at the wheel (SM 41 p. 118A).
- 1800: Furnace bought 2 bellows pipes (4 September) and then 3 bellows pipes (8 September) from Mordeca Millard (Hopewell Document 8000531 per historian notecard, but the document is not found).
- 1801: On 13 February Joseph Evans was paid £1.17.6 for dressing the bellows and £0.18.9 for work at furnace wheel (SM 100 p. 142). On 18 July Tho. Brooke?? was paid £0.3.0 "for the wool for stuffing furnace bellows" (SM 43 p. 213). Note that [Apple 1956b] on p. II-120 states "In 1801 a workman was credited for 'Stuffing Furnace Bellows'" and references "DB 1800-1802, HSP p. 213." This is a mistake; the page is the reference to buying the wool.
- 1802: On 1 Feb, Samuel Cox was paid for 1 day "cleaning stuffing & oiling bellows" (SM 100 p. 330). Joseph Evans was paid £6.0.0 for dressing the bellows in April (SM 1-2 p. 3), and £9.13.3 was paid "to his hands for sundry work at the wheale" (SM 33 p. 20). A historian notecard indicates that this was 29.5 man-days of work, sourced to SM 43 p. 20, but the microfilm is difficult to read. On 24 March Mordeca Millard did or billed for "mending a pair of bellowses" for £2.12.6 (Hopewell Document 8000531 per historian notecard). Note that KCFO attributes this to Joseph Evans.
- 1804: On April 9, Joseph Evans was paid £3.0.0 "for preparing the bellowses" (SM 33 p. 340). On 3 January 1806 he was paid £0.7.6 for "1 day work at bellows July 19th 1804" (SM 44, no page numbers).
- 1805: On 23 April Christopher Rimby dressed the bellows (SM 1-1 p. 75) and was paid £7.10.0 (SM 3 p. 181). On 22 July Jacob Moyer was paid £43.2.6 for "making the stamping mill & repairing the furnace wheel" (SM 44), and the wheel work was likely done from April

23 to May 4 (SM 1-1 p. 75, 77). On 2 December, Joseph Thomas was paid £2.1.3 for "work done at furnace wheel" and various other things (SM 44).

- 1806: On 3 January Joseph Evans was paid £0.11.3 for "1 day work at bellows" and £1.10.0 for mending the wheel (SM 44). On 6 November Joseph Evans was paid £5.0.0 for repairing the furnace wheel and bellows, during the spring (SM 4, p. 110B). Note that KCFO refers to repairs to the "bellows spring" with a minor mark between the words, while the actual entry is "Repairing the Furnace wheel & Bellows last spring". In addition, the repair description is duplicated for 1806 and 1807, but no record can be found for 1807. This is assumed to be a mistake. Also note that money amounts for these years are mistakenly entered in the "dressing" column on KCFO. On 6 November Joseph Evans was also paid £0.7.6 for "work done at the bellows" (SM 44).
- 1807: [Apple 1956b] p. II-121 states that in 1807 a worker made a cam pattern and did other work associated with the wheel and blast. Apple cites a furnace day book that does not exist, and the corresponding main journal has no such entry for the given date. A historian notecard references SM 45, which is missing, for 4 January 1808, paying Joseph Evans £3.7.9 for repairing the bellows and wheel last spring, and £1.2.6 for making a cam? pattern and repairing the wheel. This is presumed to be the Apple reference.
- 1808: On 3 October Peter Jones was paid £3.2.8 for 3 sides of bellows leather weighing $19\frac{1}{4}$, which was "delivered Mordica Millard 10 January 1807" (SM 4 p. 239B). Then on 31 December, Mordica Millard was charged £3.2.8 for bellows leather (p. 249B). Note that KCFO is indicating that this leather was bought for the furnace in 1807, but this seems incorrect, as the leather apparently was never used by the furnace. On 1 September Bernard Vanleer & Co was paid £6.0.0 "for a Pair of Furnace Pipes" (SM 4 p. 237A).
- 1816: On 5 June Rossiter was paid $9.00 "for bellows leather per John Wilson" (SM 7 p. 14B) and apparently paid an additional $15.15 "for bellows leather in June last" on 4 September (SM 7 p. 39A). On 27 June David Rutter was paid $4.58 for "bellows pipe" and $3.00 for caulking bellows pipes (SM 7 p. 20B). On 4 May Sam Harret was paid $15 for a "furnace shaft" (SM 7 p. 8A), but it is not clear what this was. On 30 May 1817 the repairs account was debited $303.78 "for Mill Wright work in the Summer of 1816" (SM 7 p. 150A). This totaled $236\frac{3}{4}$ man-days of *unspecified* mill work. There were four men involved (John Wilson, Jacob Buckwalter, Owen Evans and Jarred Evans), with work ranging from 52 to 68 days. Finally, KCFO has the entry "Leathering bellows" in 1816, but the only similar entry found in the books was in SM 7 p. 11B for 22 May, where Peter Rodarmer was paid $2.00 "for leathering a pair Smith Bellows". On 21 March Charles Brooke was paid $2.25 for "3 Skins for the Bellows", although these could reasonably be for the smith bellows (SM 7 p. 1).
- 1817: On 7 May, John Wilson was paid $14 for dressing the bellows (SM 7 p. 138B).
- 1818: On 22 October, John Wilson was paid $10 for dressing the bellows (SM 102 p. 46B). On 17 January Michael Sands was paid $\$38.49\frac{1}{2}$for building a wheelhouse (SM 8 p. 153B)
- 1819: On 30 March, Jacob Buckwalter was paid $4 for dressing bellows (SM 9 p. 152A).
- 1820: On 10 June Joseph Evans was paid $25.00 for dressing furnace bellows (SM 10 p. 58B).
- 1822: Henry Dotterer was paid $50.00 for use of his "patent elastic piston springs" in the bellows, based on his receipt dated 8 March (Hopewell Document 8220308B).
- 1824: On 29 January 1825, Thomas Kinney was paid for various things billed on 30 January 1824, including: 5 days of work at furnace by two men, $4.72; 2 days work of work at dam, $1.33; and 2 hands working 1 day at wheel pit, $1.33 (SM 12 p. 18A).
- 1825: In early April, John Keenan was paid $12 cash for repairing the bellows (SM 12 p. 33B).
- 1826: On 1 May, James Everhart was paid $35.45 for "leather harness bellows leather & sheep skin" (SM 12 p. 144A). "McKerson dressed & repaired F. bellows" and was paid $18 (KCFO). The difference between dressing and repair is not clear, so this will be split as $9

for repair and \$9 for dressing. On 29 April Samuel Koplin was paid \$1.75 for unspecified millwrighting (SM 11 p. 264). See 1827 for millwrighting possibly done in 1826 instead.

- 1827: In January, Henry Volkmar was paid \$18.90 for 2 bellows pipes, for $31\frac{1}{2}$ lb at 60 cents/pound (SM 13 p. 13B). In March 1828, Jacob Miller was paid per his account for various work done between 7 August 1826 and 26 November 1827 (SM 13 p. 19C). This included $45\frac{3}{4}$ days of millwrighting work done by himself and three other men, the longest duration being 20 days. In addition there were 32 additional days of carpenter work that may have been associated with millwrighting, with the longest duration being 31 days.
- 1828: On 14 May the furnace paid three men for a total of $70\frac{1}{2}$ man-days of work at the furnace stack, ranging from $19\frac{1}{2}$ to 26 days (SM 13 p. 23A). On 29 August James Everhart was paid \$30.39 for "harness bellows leather & hair" (SM 13 p. 31B).
- 1829: On 11 May Henry McMurfy was paid \$15.00 for dressing the furnace bellows (SM 80, no page numbers).
- 1830: Joshua C. Wright was paid \$35.00 "for Dressing the Bellows & Putting in his Improved Pattent [sic] Springs" (SM 15 p. 15A, dated 29 November 1830). The breakdown is not specified. Most carpenter and mill work appears to go for less than \$1 per day, and spring replacement should be doable in less than two weeks, so allocate \$10 to the spring work and \$25 to the dressing. The furnace bought "a Sett Bellows Springs" for \$1.50 from Birdsborough Forge, sometime between 26 April 1830 and 1 April 1831 (SM 21 p. 23A). It is not clear whether these are the above springs. Jacob Miller was paid \$9.42$\frac{1}{2}$ for 12 man-days of unspecified millwright work by himself and three others (maximum $4\frac{3}{4}$ days) between 5 April 1830 and 1 March 1831 (SM 21 p. 22A). He was also paid \$6.07$\frac{1}{2}$ for $5\frac{1}{2}$ man-days "hewing shaft" by himself and two others (maximum 2 days). This is presumed to be for the new wheel (SM 21 p. 22A).
- 1831: Joshua C. Wright was paid \$16 for dressing bellows in the spring (SM 21 p. 14B). Jacob Miller was paid \$59.25 for 78 man-days of work by himself and two others "from the 4th of April to the 12th of May 1831 for work done at the new wheel" (maximum 32 days) (SM 21 p. 22B).
- 1832: On 27 September Samuel Knaur was paid \$0.80 for sawing "200 feet Stirrup Stuff" (SM 21 p. 40A). On 20 May 1833 Robert Wright was paid \$23.00 for "Dressing the Bellows twice & putting in new stirrups in 1832" (SM 21 p. 68B). The breakdown is not given. As related later, stirrups are probably fastening devices, so their replacement is probably only a few days, so allocate \$3 to replacement \$20 to the two dressings at \$10 each. On 1 May 1833, James Everhart was paid \$16.24$\frac{1}{2}$ for bellows leather and \$0.31 for hair, delivered sometime from 27 May 1831 to this date (SM 21 p. 63B).
- 1833: On 6 June Robert Wright was paid \$70 "for Putting up the Bellows & dressing them, Putting up Paicers etc this spring" (SM 21 p. 72B). The breakdown is not given. To be consistent with dressing payments in nearby years, allocate \$10 to the dressing. On 9 April 1834 Isaachar Pawling was paid \$14.00 for "Making Sundry iron for the Bellows & Pacers" sometime between 19 June 1832 and 5 April 1834 (SM 21 p. 99B).
- 1834: Robert Wright was paid \$9.00 "for dressing the Furnace Bellowses in Jany last" and \$4.00 for "Making Patterns for blacking Roler etc & putting up the same" (SM 21 p. 95a, dated 13 March). He was further paid \$12.00 for dressing the furnace bellows this summer; and \$12.69 for $14\frac{1}{2}$ days "work at the new trunk & makeing Cam?ing pattern etc etc by himself & Anderson" (SM 21 p. 108A, dated 13 August). KCFO describes part of this as "camting (?)," but based on other hand writing, the most likely interpretation is a "cam ring pattern" (there is also a line break at the uncertain character). David Lockhart and Isaac Wynn were paid \$31.31 for 14 and 13 days millwright work "at the shaft etc" from 25 July to 9 August (SM 21 p. 108A). Isaac Hughs was paid for $2\frac{1}{2}$ days labor "when furnace shaft broke" (SM 21 p. 118B). Robert Neeley was paid \$16.00 "in full for a Furnace Shaft the 29th of July last" (SM 105 p. 75). A tree for the shaft was cut around 26 July (Hopewell Document 8340726A). The cost of trunk is not clear but may be $23\frac{1}{2}$ lb at \$19.89 (KCFO).

There were $23\frac{1}{2}$ man-days of unspecified work at the wheel for $19.89, and "$6\frac{3}{4}$ d. at w. pit" (KCFO).

- 1835: On 1 January, George North was paid $7.20 for "9 days work at the new trunk @80" (SM 21 p. 118B). Robert Wright was paid $12.00 for "dressing the Bellowses last Spring". This is by SM 21 p. 135B which is dated 28 April 1835, so this is taken as having been done in 1835. On 1 May, James Everhart was paid $14.56 for "2 sides Bellows Leather 52 @28" (52 lb at 28 cents per pound) and $0.75 for "finishing the same". (SM 21 p. 137A). On 2 June, Elisha R. Sands was paid 7.59\frac{1}{2}$ for $6\frac{3}{4}$ days "work done at the furnace trough from the 30th of July to the 9th of August 1834" at 1.12\frac{1}{2}$ per day (SM 21 p. 142A).
- 1837: On 10 November John K. Wright issued a receipt for payment for services, which included what might be "setting a spring" (Hopewell Document 8371110A).
- 1838: On 24 December Isaac Markley was paid $287.74 for "Mill Wrighting etc" (SM 38 p. 188). A historian notecard claims that "piston stems" were bought from Birdsborough Forge this year, referencing Hopewell Document 8381109, but the only document found is 8381109A, which does not contain any such entry.
- 1839: On 20 June Augustus Leopold paid Philip Lott $10 (presumably in cash) "for forging 2 pisten rods for Hopewell furnace" (Hopewell Document 8390620). The transaction does not appear on the Hopewell books, and could have been payment for work done in the past.
- 1840: W. W. Weaver was paid $2.00 for turning a screw on the piston stem of the bellows (SM 28 p. 25B). Isaac Markley was paid 3.12\frac{1}{2}$ for his brother Joseph dressing bellows. (SM 28 p. 82B).
- 1842: Joseph Markley was paid 6.87\frac{1}{2}$ for dressing the bellows (SM 28 p. 82B).
- 1843: Isaac Markley was paid $5.00 for dressing the bellows "for this year" (SM 28 p. 96B).
- 1844: Isaac Markley was paid $2.00 for dressing the bellows (SM 28 p. 123B).
- 1845: Isaac Markley was paid $5.00 for dressing the bellows (SM 28 p. 152B). E.&C.B. Grubb was paid expenses of $2.79 on "hot blast pipes as of receipt" (SM 28 p. 152B). Christian Snagle was paid $1.50 "for Repairing Furnace pipes" (SM 28 p. 163B).
- 1846: Joseph Markley was paid $2.50 on 8 April for dressing the bellows (SM 31 p. 9B).
- 1847: Isaac Markley was paid $2 for dressing bellows (SM 31 p. 30B).
- 1848: Christian Snagle was paid $7.46 for mending the wheels on the horse power at the mineholes and fixing the furnace copper pipes (SM 31 p. 63B). Isaac Markley was paid $44.00 "for work done in putting up a new trunk & dressing bellows in May 1848" (SM 32 p. 59A). The payment is broken down by worker but not by function. KCFO is implying that $41 is for the new trunk, and $3 for dressing, but the source is not clear. This was a total of about 41 man-days, done over $1\frac{1}{2}$ weeks.
- 1849: On 16 March James Everhart was paid $15.44 for "bellows leather & boot leather" (SM 32 p. 62A). On 1 June, Isaac Markley was paid $14.25 for $9\frac{1}{2}$ days of unspecified millwrighting. In addition, he appears to have been paid for another 15 man-days of millwrighting done by others. This is a total of $24\frac{1}{2}$ man-days done over around 10 days (SM 32 p. 68B).
- 1850: Geiger was paid $0.50 for mending a tin blast pipe (SM 31 p. 127B). Joseph Markley was paid $3.00 for repairing the bellows (SM 31 p. 133B). On 30 April James Everhart was paid $11.50 for 35 lb "bellows leather and finishing" (SM 32 p. 89B).
- 1851: Buy $1.37 of copper pipe, but unclear if this is related to the furnace (SM 31 p. 138B).
- 1852: On 30 September Isaac Markley was paid $3.00 for "2 days repairing tubs & c @1.50" (SM 32 p. 133B).
- 1853: On 28 March, Hashabiah Clemens was paid $1.25 for "Making Boot for B. Pipe" (SM 32 p. 141B). On 31 March, Joseph Markley was paid $52.50 for 42 days of unspecified millwrighting, plus $24.75 for $34\frac{3}{4}$ days of presumed millwrighting for Sheeler (SM 32 p. 142A). This is $76\frac{3}{4}$ man-days of millwrighting over about $1\frac{1}{2}$ months.

- 1854: On 18 August “Paid I? Markley for repairing blowing cylinder[s]”, $13.00, with no mention of which furnace (SM 65 p. 1B). KCFO has repair of blowing cylinders, but marked for “ANTH FURNACE”.
- 1855: On 26 January Adam Gotwals was paid $53.77 for 3 tuyeres and gas pipe (SM 65 p. 11B).
- 1866: The furnace blowed out on Christmas night, 25 December, “wheel broke down” (SM 60 p. 6B).
- 1869: E&G Brooke was paid $21 for “babbit metal & straightening piston rod” (SM 34-1B p. 194b). Note that Hugins on p. 11 states that metal piston rods replaced wood ones in 1869, referencing day book 1851-1883, p. 195b, which is SM 34-1. Page 195b has nothing relevant and this is assumed to mean p. 194b. It is concluded that Hugins overstates the evidence, and no replacement was done.
- 1872: On 21 August a Markley was paid $3 for repairing bellows (SM 62 p. 5)
- 1873: On 14 May paid $1.08 for babbit metal (SM 62 p. 27). On 25 October paid a Markley $11 for “Repairing tubs” (SM 62 p. 59).
- 1874: On 28 November paid $22.50 to Isaac Wynn and Jos. Markley for 9 days repairing bellows (SM 62 p. 109). On 16 December paid $11.52 for 72 lb babbit metal and $0.60 for 2 lb brass (SM 34-1B, p. 233A).
- 1876: Paid E&G Brooke: $1.00 for “turning wrist”; $0.25 for “work on pattern” and $13.20 for a 440 lb “crank casting” (SM 34-1B p. 242A).
- 1877: On 4 August the furnace SOLD $5\frac{1}{2}$lb bellows leather to Knauer & Co for $2.75 (SM 122 p. 56).
- 1878: On 1 January 1879, payment was made for “work at water trunk”, $6.30 for 7 man-days total (John Care for 2 days, Nathan Care Jr for 2 days and H. A. Long for 3 days) (SM 122 p. 76).
- 1880: On 30 June 1882 Wilson Simmers was paid $59.00 for $29\frac{1}{2}$ days “work at furnace” in December 1880 and January 1881 (SM 122 p. 174). In August, smithwork included “work for bellows”, $5.50 (SM 34 p. 215). Also “pipe for bellows” and “pipe”, “blast pipe cut” (KCFO). [Long 1930] on p. 6 claims that the wheel froze in January; this implies installation of the backup engine.
- 1881: On 15 October Wm. Durell was paid $5.25 for “$3\frac{1}{2}$ days work while repairing fur”, where “fur” is assumed to be furnace (SM 122 p. 151). On 30 June 1882 Wilson Simmers was paid $6.00 for 4 days work done repairing in 1881, where the context implies repairing the furnace, and Wm. Sheeler was paid $57.79 “for bill of lumber while repairing furnace in January 1881 for ditto in October 1881” (SM 122 p. 174). By [Kemper 1936], Long claims the wheel froze on January 1, leading to the installation of the backup engine. R&A p. 52 states that in 1881 a larger receiver was installed between the blowing tubs; this presumably comes from Kemper (hence Long), who said “much larger.” A historian notecard claims that the earlier receiver was still at Hopewell.
- 1882: About the end of the year Col L. H. Smith was paid $4.50 for 3 “tu[?]yeere irons(?) at $1.50 each” (SM 34-2 p. 263a). “Smithing: 3 bands on pipe” (KCFO).

These entries are discussed in the sections below.

7. The Final/Current Blast

The current (2016) blast should be an almost exact copy of the 1883 blast, and according to Cass, part of the current blast *is* what was operating in 1883. Hence the description of the current blast is part of the result of the overall investigation into the blast design over time. It also provides a reference for interpreting statements and data concerning prior blasts.

According to Cass on pp. 10–11, one tub and the receiver are from 1883, although with restoration work. The other tub and both pistons are “new” (1950). Since the restoration and new construction is said to follow the originals, examination of the current blast tells us what the final operational blast was like. In addition to the Cass/Hugins report, the blast is described by drawings 2062 and 2062A (approximately the same) [Higgins 1949] which appear to be the reference drawings for the restoration and construction of the wheel and blast. The original drawings by the Franklin Institute are in [Bonnie&Costa 1931].

This description comes from pp. 10–11 of Cass and from the drawings, unless otherwise stated. The narrative from the report is reproduced in the appendix in Section 20.

The tubs (cylinders) are made of wood staves and are 4.5 ft high and about 6 ft in diameter, and said to be "perfect" cylinders. They are glued and further held together by metal bands. The inside walls are sanded and painted for smoothness. The stroke is 31 in based on an December 2015 measurement of the north crank arm.

The pistons are built out of 4 layers of wood with a nominal thickness of $6\frac{1}{8}$ in. All cracks and joints are sealed with glued strips of canvas. The primary piston/cylinder seal comes from a strip of $\frac{1}{4}$ in thick leather that is $7\frac{3}{8}$ in wide and nailed to the circumference of the piston. This strip is actually nailed over further leather blocking strips that are $\frac{1}{2}$ in wide and also $\frac{1}{4}$ in thick. Note that the circumference strip is directly backed by only one of the four piston layers, in order to permit the remainder of the leather to be pushed outward by piston rings; see sheet 4 of the drawings.

The piston seal is accomplished by attaching the leather strip only at it's center and using two piston rings to force the top and bottom sides outward into the cylinder. A ring is Ash and $\frac{1}{8}$ in thick and $\frac{3}{4}$ in tall. Each of the top and bottom rings is made of two strips each $8\frac{1}{2}$ ft long. It appears from the drawing that the two strips have a gap between them at both ends, into which the two piston springs go. The springs are metal and appear to be circular when released, and installed with leather holders such that the two ring strips are pushed apart from each other, and hence outward into the leather strips.

Each tub has a top and bottom intake valve, four valves total. This is a wood "trap door" valve. The opening is a rectangle 15 in by 19 in with a beveled edge. The valve door swings on hinges to close against this bevel. The door is faced with $\frac{1}{8}$ in leather for a seal. Each door is connected to a "valve float," a wood piece on an axle that is weighted so as to offset the door weight, and permit it to be pulled open by suction, and shut by compression.

Each tub has two pipes to the air chamber, also variously called the receiver or mixing box (regulator in Britain). These are simple flap valves that are opened by pressure from tub air and automatically closed by both gravity and receiver pressure. Each is made from wood and faced with $\frac{1}{8}$ in leather for a seal. The receiver has an access hole whose cover is faced with $\frac{1}{8}$ in leather for a seal. Unlike some historical blasts, this receiver does not have a weighted piston for pressure equalization.

According to [Apple 1956b] on p. II-135, the blast was restored in 1952. It must still be determined in which years the wheel was actually running. Recently, the wheel is run continuously from approximately spring through December. Management desires to not run the wheel when there is any chance that ice might create balance problems and cause spray on visitors. It is started in March or April when any threat of ice is past, and when possible, run through the Christmas holiday period. This means that current use approximates historical use, although historical blasts were somewhat longer, and hence current maintenance is an indication of period maintenance. It is possible however that current maintenance underestimates what was done in the period, since today the wheel is run only fast enough to blow a small cupola. However, all the current maintenance appears to be simple and fast, the sort of work that would be done by normal operating personnel, possibly without shutting down the wheel. This could reasonably eliminate its mention in the furnace books.

A potentially important question is the history of piston leather replacement, meaning, how long does the leather last? If the most recent replacement is taken as 2010, then a 30 year life would be 1980, while the blast was in modern operation. If there was no replacement in this time period, then it is plausible that a rebuild ca. 1850 could have lasted until shutdown in 1883 without needing "bellows leather," hence explaining the lack of mention in the furnace books during this period. However, a search has failed to find any contract documents for earlier modern-period blast work.

7.1. Leather Needs

One of the uncertainties in analyzing the blast data is knowing what the documented purchases of leather might have been used for. One constraint on this is how much leather is in the current blast. This is totaled here based on the description and drawings.

The main piston leather strip is almost 19 ft long, and about 12 ft^2 are required. It appears that the leather blocking for same is probably continuous around the circumference, at about $1\frac{1}{2}$ in tall, adding another $2\frac{1}{2}$ ft^2. So the pair of pistons requires about 29 ft^2 of 16 oz leather. The tub intake valves require about 2ft^2 of 8 oz leather each, for a total of 8 ft^2. Each receiver valve requires about 1 ft^2 of 8 oz leather, for a total of 4 ft^2. The receiver access hole cover requires about 2 ft^2 of 8 oz leather. It appears that each end of each pipe calls for a $1\frac{1}{2}$ in wide strip of 8 oz leather, for a little over 3 ft^2 total.

The Cass report describes releathering the interior of the receiver. The meaning of this is unclear. It could mean replacing the documented leather seals that are on the interior. It could also mean, in addition, lining all the interior wood with leather as an air seal. However, the drawings do not indicate the latter. It seems more likely that canvas tape was glued to the board joints as with the pistons instead. It appears that no one has seen the inside, and opening the receiver is not considered desirable. It will be assumed that this lining does not exist.

The initial construction required 29 ft^2 of 16 oz leather and 17 ft^2 of 8 oz leather. Ignoring excess required for unusable areas of a hide, this requires about 32 lb of 16 oz leather and 10 lb of 8 oz leather, or 42 lb total.

8. The Blacksmith Bellows

The blacksmith bellows complicates any analysis of the furnace blast due to the possibility that some book entries are actually for the blacksmith bellows. Hence knowledge of same is necessary to be able to tell if confusion of the two is plausible. We also want be confident that the historical blacksmith bellows currently in storage is the same or same type as was used in the period, so that knowledge of that informs us of the bellows used over time. (The bellows currently in service in the blacksmith shop is said to be a modern replacement.)

8.1. Blacksmith Shop History

The construction date for the shop is not known, although it was apparently built early in the life of the furnace. Motz [Motz 1940] on p. 17 directly states that the shop was probably built by 1775 or 1780. (Motz did the archeology on the shop.) Heydinger [Heydinger 1965] on p. 1 states that the construction date of the blacksmith shop is said to be unknown, although the shop was "Seemingly operating in 1784". Albright [Albright 1974] on p. 15 states that "No exact date for the construction of the blacksmith shop has yet been discovered." He quotes Motz on p. 5 as saying that the shop sits on slag, indicating that the furnace was in operation before the shop was built. On p. 16 he quotes Motz as saying the shop was constructed 1770–1800, more likely toward 1770.

At the other end of the shop's life, Motz on pp. 18–19 states that the shop was renovated in 1849 due to the construction of the anthracite furnace, and that "The present bellows, crane and drill press were installed". (He was writing ca. 1940.) He also states that since 1883, there were no major repairs, only slow decay. Albright on pp. 19–20 also states that extensive renovations were done to the shop in 1849 due to needs created by the construction of the anthracite furnace, and that “The 1849-era renovation is essentially the shop seen today.” Gale on p. 3, also writing about 1940, states that “bellows found in the shop ... while in a good state of preservation to be used as a museum piece, should not be put into service.” Albright on p. 25 states that the bellows is from the mid-1800’s. I take this to mean that the bellows found in the 1900’s during investigation and restoration dated from the mid-1800’s. The only issue is that Albright made that statement ca. 1974, which seems to be after the point at which the historical unit would have been replaced. Aside from minor uncertainty with Albright’s last statement, the historians and archeologist seem to agree that the bellows they found was the same one or same type that existed ca. 1849.

8.2. Blacksmith Bellows History

There is also some record of work on the blacksmith bellows. On SM 100 p. 239, for 21 August 1801, Mordica Miller is paid £7.10.0 for “one smith bellows”. Heydinger states on p. 1 that the bellows was repaired in 1808, referencing SM 4, pp. 155A and 125B. P. 125B of SM 4 is for 16 December 1806, not 1808. This page does have an entry for paying Mordica Millard £2.8.6 for “Making and Repairing Smith Bellows.” This has some significance, as it shows some propensity for the clerk (at least of this time) to differentiate between the blacksmith and furnace equipment. P. 125A has no bellows-related entries. P. 155A is for 4 April 1807 and has nothing related to a bellows, but does pay George Jones £0.0.11 for fetching a “smiths nice” (?) from Philadelphia. P. 155B has no related entries. So in spite of the problems with the citations, work of some sort was done on the blacksmith bellows in 1806. Heydinger states on p. 1 that the bellows was releathered in 1816 at a cost of $2, referencing SM 7 for 22 May 1816. This reference is accurate. P. 11B of SM 7 is for 22 May 1816 and contains an entry paying $2.00 to Peter Rodarmer for “Leathering a pair Smith Bellows”. Again, note the direct reference to “smith.” Why there is a “pair” is unclear; this could be a reference a double-acting bellows, or perhaps he also worked on a unit from some other facility.

Albright on p. 26 states that “The shop’s bellows required releathering in 1806 and 1816”, referencing SM 4 p. 155A and SM 7 22 May 1816. The 1806 work is clear, but Albright’s claim of releathering is discounted as the book does not support this (but does not rule it out). The 1816 releathering claim is supported. He further states that “In 1819, a bellows was purchased for $28.50” referencing p. 134 of SM ?. The reference is to p. 134B of SM 9, for 8 February 1819, where Jesse L. Stillwagon is paid $28.50 for “1 Smith Bellows”. In an entry in SM 105 p. 19 for 31 October 1833, the general Furnace account pays Eckstein $25.00 “for smith bellows.” Hopewell Document 8331025A is a receipt for this, “one 36 in Bellows”, dated 25 October.

The 1816 work is presumed to be part of the general restoration required after the eight year furnace shutdown. For whatever reason a new unit was purchased in 1819, and lasted 14 or 17 years. A similar lifetime would bring this one to the 1849 shop renovation. Between this and the previous statements that the ca. 1849 bellows was found in the 1900’s, we have a chain indicating continuous use of a leather accordion bellows for the blacksmith shop. There is unfortunately a lack of record for the remaining furnace life. A releathering in 1849 and one more would last until the end of operation assuming a 17 year life of the 1819 bellows. As described below, relatively little leather is required for the blacksmith bellows, so it is less surprising that it might be missed compared to, say, leather for a pair of leather furnace bellows.

8.3. The Historical Blacksmith Bellows

The current (2016) bellows in the shop is a modern replacement, although one comment by Albright raises the question as to when it was made. The bellows found when the site was acquired, the one that was replaced by the current bellows, is stored at Hopewell in the Museum Storage Building, and was accessioned on 16 September 1940. It was examined on 4 October 2015.

Consistent with descriptions [Gale 1941], this appears to be a double-acting unit, with support pegs set into the center board. The dried and curling leather was accessible in places and was measured with a 0.001 in resolution caliper, and found to be around 0.05 in thick. This would be consistent with 4 oz leather. The unit is about 54 in long and 36 in at the widest point, consistent with the 1836 purchase. These general dimensions are about the same as in an NPS drawing [NPS 1940]. The back is circular, and the perimeter from the nose to the midpoint of the back is about 60 in. There are a total of 4 folds in the leather, 2 each above and below the center board. I estimate that each fold is about 4 in high at the back, with an additional 5 in between and around the folds. This implies about 21 in of leather height at the back. The nose is about 6 in high.

A crude estimate of leather requirements starts by taking the back as a half-circle 21 in high and of 18 in radius. This requires about 8 ft^2 of leather. Each side is a trapezoid requiring about 4 ft^2. So the entire bellows could be leathered with approximately 16 ft^2. If it was desired to cut two pieces out of larger rectangles, 20 ft^2 might have been purchased, allowing some to go to waste. In either case, only 4-5 lb of leather is required assuming a 4 oz weight.

8.4. Blacksmith Bellows Summary

There are several relevant points from the above information. First, the blacksmith bellows appears to have been the classic leather accordion type for the life of the shop. Second, we likely have the unit that was in use in 1883, which tells us what they were using. Third, leather needs were slight compared to what went into the furnace blast, so the blacksmith bellows could have been serviced from leftovers from any large furnace blast leather purchase. This could also partially explain a lack of "smith bellows" records after 1836; possibly the leather was "free" and only the work of installing it went unrecorded. Finally, the known history of purchases specifically for the blacksmith shop gives confidence that the shop was taken care of without the requirement that any of the data attributed to the furnace blast was actually for the blacksmith bellows. Also note that there are various main journal entries for "soal leather" and "upper leather" and others, giving additional confidence that leather usage was generally identified.

9. Discussion of Springs

The meaning of "springs" is of particular importance for deciphering blast design. The appearance of springs may or may not signal the conversion from a leather-based accordion blast to a cylinder blast, and knowing the details of springs in use at a specific time might prove, or at least constrain, the design of the blast they were installed in. The problem is that the generic term "spring" has an uncertain meaning, including to past Hopewell historians. While the installation of Dotterer's "piston springs" in 1822 is taken by some historians as conclusive that a tub-type blast was installed by 1822, "Archeologist Leland Abel, on the other hand, thought the tubs were installed around 1851." (Yocum p. 47).

A spring could have been used with leather bellows, the current blowing tubs, something in between, or all of these. There are the Dotterer "piston springs" in 1822, Wright's "Improved pattent springs" in 1830 and "bellows springs" in 1830–1831. Either the blast did not change or these are different types of springs. The mention of "bellows springs" after "piston springs" is a particular problem, and the fact that the term "tubs" is not used until 1852 adds to the uncertainty.

This section evaluates evidence for the period meaning of the concepts listed here, to see if any conclusions can be drawn as to what implications the various mentions of "spring" have to blast design. Of concern is:

- The phrase "piston spring."
- The phrase "bellows spring."
- The phrases "elastic piston" and "elastic piston spring."

- The exact nature of the springs installed, including the current ones and those that a patent fee was paid for.
- Historical spring terminology in general.
- The historical use of springs in blast equipment.

9.1. Historical Spring Use in Blasts

There are no references to springs in any of the blasts described in the history section. Further, return actions in the described blasts were by gravity, counterweight or direct action, and a spring could replace gravity or counterweights without modifying the general design of a blast. So the lack of spring mention has limited meaning for interpretation of later blasts.

Note however that springs were used to reinflate organ leather bellows in the mid 1800s, as described below in the section on “bellows springs.”

9.2. Springs in the Current Blast

The current blast was described in detail above. This contains metal springs, based on working drawings [Higgins 1949]. The springs force the piston rings outward from the piston against the cylinder wall, to provide an air seal. This usage is one definition of the phrase “piston spring,” but unfortunately, not the only one.

9.3. The Dotterer Patent

The installation of Henry Dotterer’s “patent elastic piston springs” in 1822 is potentially significant, although still uncertain, since the exact meaning of this phrase is not as clear as it might seem, as will be discussed below. The receipt given by Henry Dotterer for use of his invention has no description of the nature of the invention.

Finding a patent drawing or description might remove uncertainty as to exactly what this invention was, and hence inform the blast design of the time. According to two patent indices [Burke 1847 and Leggett 1874], there was indeed a Henry Dotterer from Philadelphia who patented three inventions related to bellows in this time period: a 16 February 1809 patent X1,005 for “Bellows, pump, for forges;” a 1 May 1810 patent X1,300 for “Bellows, elastic piston;” and a 24 April 1825 patent X4,086 for “Bellows, furnace.” (The last may have been issued on 14 April.) None of these has a patent number listed in the indices, however, the Directory of American Tool and Machinery Patents contains the specified numbers.

Few patents from this era (“X-patents”) are available from the USPTO due to the 1836 fire, and none of the Dotterer patents are present. At this point no information is available on his patents except the one-line descriptions from the indices as quoted above. The 1809 patent is probably not applicable, and the 1825 patent is too late. The 1810 patent is presumably the one referred to by the 1822 work, and some effort has been made to find information on it.

Searches of common science and historical databases and Google Scholar did not produce any information. Two early journals related interesting patents, and are known (potential) sources for X-patents: the (now) *Journal of the Franklin Institute*, starting in 1826, and *Mechanics Magazine*, starting in 1825. No references to Dotterer patents were found. (Interestingly, the very first issue of JFI did contain a comment by the editor slamming the issuance of patents for useless and obvious “inventions,” so much so that to relate all of them would be to create a “manufactory of waste paper.”)

Additional searching was done on a full-text index of old newspapers, on the possibility that Dotterer advertised. (This is not assured; Hopewell sold through Philadelphia dealers, so there could have been other routes to learn about a Philadelphia invention.) The "Nineteenth Century U.S. Newspaper Digital Archive" indexes old papers for full text, including the Philadelphia area "Aurora & Franklin Gazette" and the "Aurora & Pennsylvania Gazette." Searching for "henry" and "dotterer" got unrelated hits in the 1825 A&FG and one hit in the 17 Jan 1829 (issue 15) A&PG, in the "letters remaining at post office" section. No hits were found for "elastic piston." 10 hits were found for "piston spring" but none were relevant. The first was in 1842 for a "metallic spring piston head," which seems to be packing, being compared to hemp piston packing. There were 21 hits for "bellows spring," but none were relevant.

In addition, there are a series of papers published in Philadelphia from 1810, under names that include "General Advertiser" and "Philadelphia Aurora." The GA is available in image form at `viewshare.org`, but I have not found a full-text source. This has not been reviewed.

Searching in the *NewsBank* and *Ancestry Library* databases did not turn up anything useful.

Searching for the Dotterer patent has found nothing.

9.4. Possible Dotterer Tubs

There is an interesting description of the Chestnut Grove Furnace on p. C. 243 of [Frazer 1877]. This was near Whitestown (Idaville) in Adams County, Pennsylvania (a bit north-west of Gettysburg). According to the account, this furnace was built in 1837 by Duncan & Mahon, and was 30 ft high and 8 ft diameter at the bosh, similar to Hopewell Furnace. To quote from the account, "It was used solely as a cold blast furnace, and was blown at first by the old Dottener tubs, which only blew one way." The spelling of "Dottener" is correctly transcribed, with an "n" instead of a "r." A plain Google search does turn up various Dotteners in the 1930 and 1940 US Censuses, but this still seems more likely to be an error than to have both a Dotterer and a Dottener making blast machinery in the same area at the same time, and both known by name for their work.

Assuming that this is the same Dotterer that had the spring patent, this provides some information. This directly states that a tub blast was associated with Dotterer's work. It implies that his blast design was still used in 1837, and probably that it was considered "old" at the time. This would be consistent with it being worthy of an upgrade to it in 1822. If we take the statement to imply that a particular cylinder design was associated with Dotterer, and assume that installation of his springs at Hopewell implies that the Hopewell blast was at least consistent with his usual design, then the statement is also implying a single-acting blast.

In general, the statement is evidence that single-acting blowing tubs were in use at Hopewell in 1822.

9.5. A Possible Wright Patent

On 29 November 1830, Joshua C. Wright was paid $35.00 "for Dressing the Bellows & Putting in his Improved Pattent [sic] Springs" (SM 15 p. 15A). Possibly related to this, sometime between 26 April 1830 and 1 April 1831, the furnace bought "a Sett Bellows Springs" for $1.50 from Birdsborough Forge (SM 21 p. 23A). It is not clear whether these are the same springs. However, the low cost indicates that there is not much to them. Note that the book entry was only for installation, and the lack of a license payment is odd in comparison to the Dotterer case. Regardless, I did patent searches related to him.

Given the 1830 installation date, any related patent would be an X-patent. Searching the X-patent list turns up nothing for anyone like Joshua Wright, and none of the inventions by any Wright use a spring, bellows or piston. So it appears that the claim of him being a pantentee is a mistake. It is also possible that blast piston springs at the time were generically called "pattent springs."

I also searched the Digital Archive for any newspaper information. Using "joshua wright" got 68 hits. The 11 in classified were inspected and none were relevant. There were several in March 1826 in the A&FG, for unpaid taxes. Finally, there were some in the 1828 A&FG, in the letters at PO section. So it appears unlikely that Wright was either an inventor or doing any general advertising for components or services for blast machinery.

Given the limited information from furnace records, it appears impossible to determine what sort of springs were installed in 1830.

9.6. General History of Springs

An examination of what general spring terminology meant in the period was attempted through a combination of literature and patent searches. Patents were generally found through a list of X-patents (described below) and Google Scholar. In addition, searches were done on the databases *America: History and Life*; *History of Science, Technology and Medicine*; and *Web of Science*. Very little was found in the general searching on spring history. The most relevant item was a history of the spring industry [Fawcett 1983], whose initial chapters are a more general history of springs. The author, then the head librarian at the Field Museum, stated that he also found little on spring history. On p. 13 he states that the *The Cyclopaedia; or, Universal Dictionary of Arts, Sciences, and Literature* from 1819, has little to say about springs, placing them in watches, and "a lock, pistol, or the like." On p. 23 he states that "The decade of the 1850s may be regarded as a key period in the history of the springmaking industry in the United states." So there seems to be little available for the first half of the nineteenth century.

However, the initial chapters of Fawcett offer some useful insight into period terminology in the form of a list of all the terms used and their associated meanings. This is a list of spring descriptors found, indicating some of the technology seen at the time, and possibly the period terminology:

- Helical spring and helical torsion spring.
- Leaved spring, as used in a crossbow.
- Spiral spring, also a clock spring or flat coil spring, first mentioned around 1500. He says that this period is possibly the first application of flat coil springs to clocks. A flat spring is the same as a leaf spring, at least for clocks.
- Balance spring or hairspring, for clocks.
- Leaf springs (in Europe starting in the 1500s).
- Elbow springs, but these are not described.
- Carriage springs
- Cylindrical and flat-helix hair springs.
- Conical or straight springs and conical spiral springs.
- Coiled springs.
- Wire springs.

From this it is taken that a spring descriptor can mean several things: the style (i.e., coil or leaf); the shape (i.e., wire or flat); or the application (i.e., clock or carriage).

The next step was a search of the X-patents to see how terms were apparently used at the time. This used Jim Shaw's list of X-patents [Shaw 2xxx]. There were no hits for "spring" and any of these terms: "piston," "helical," "cylindrical" or "coil." There were 12 hits for "spiral" and "spring." All are apparently referring to a shape of spring, i.e., spiral wound instead of flat.

There were 125 hits for just "spring." Most (109) were for the application of a spring to something, such as a carriage, pump, etc. 3 concerned the type of spring that produces water, 3 concerned making springs, several seemed to be about springs themselves, and the rest were unclear.

To get a further reading on the use of terminology for spring type, such as "coil spring" to indicate use of a spring that has a coil form, searches were done for several known types. Searching the above databases turned up little, so Google Scholar searches were done instead. These turned up mainly patents in earlier years. Google Scholar searches were done for the time ranges 1837–1870 and 1871–1925. The former starts immediately after the X-patent period. In all cases the search was for "spring" and the type word. These are shown with the X-patent counts in Table 1. The term "cylindrical spring" is not well used but generally seems to be the same as a helical spring. The pattern is clear, with an increasing number of references to each spring type with time. This indicates a clear use of the spring-type terminology, which will be compared to the use of "piston spring" in a later subsection.

Range	**Spiral**	**Coil**	**Helical**	**Cylindrical**
X-Patents	12	0	0	0
1837–1870	4,720	336	643	26
1871–1925	15,400	15,300	12,000	327

Table 1. Search Hits for Various Spring Types.

9.7. Elastic Spring

This phrase is of interest due to the use of the phrase "elastic piston spring" associated with the 1822 installation. The parsing is unclear, as to whether this is a spring associated with an elastic piston (whatever that was), an elastic spring (whatever that was) associated with a plain piston, or an elastic spring of a piston type (whatever that was).

The list of X-patents was searched again. There were seven hits for "elastic" and "spring:"

- elastic trace and brace by connected springs
- elastic spring sofa
- elastic or spring carriage seat
- elastic spring girth and saddle tree
- making elastic spring cushions
- elastic spring bed with an elevating and depressing surface
- elastic spiral springs applied to traces, swingletrees, etc.

The word "elastic" appears to have no special meaning when associated with "spring;" it seems to be a redundant term for "springy." Note in particular the "elastic *or* spring" carriage seat. Also note that the usage of "spring" is similar to the summary from Fawcett's book, with the additional usage of "contains a spring," such as a "spring sofa."

Searching for "elastic" and "spring" in the title produced no hits in either of the history databases. *Web of Science* had 10 hits for 1900–1950 when searching by topic, and in all cases the term "elastic" simply referred to the property of elasticity. Webster's 1828 dictionary [Webster 1828] was used to get a contemporary definition of spring. There were many, including "An elastic body; a body which, when bent or forced from its natural state, has the power of recovering it; as the spring of a watch or clock." Again, "spring" and "elastic" seem to be references to the same concept.

There are modern references to the use of "elastic" with springs which may or may not reflect the same understanding as in the period. Most simply, various Wikipedia articles concerning springs describe them as being made out of an "elastic material," a modern suggestion that "elastic" is of no import in describing the spring type. Today, "elastic" also has a technical definition for springs, as in [Avallone 1978] on p. 70, in the preface to the discussion of springs: "It is assumed in the following formulas that the springs are in no case stressed beyond the elastic limit (i.e., that they are perfectly elastic) and that they are subject to Hooke's Law." This is the idea that it is, and possibly was, deemed important to remind the user that the spring does not permanently deform. Perhaps springs of the period were of uncertain quality and liable to deform after use, and there were explicit claims that the spring would keep "springing" indefinitely. In this case, the term "elastic" is simply a claim of quality and says nothing about the type of spring.

In summary, between both period and modern usage, it appears that the term "elastic" can be ignored when applied as a descriptor to springs.

9.8. Piston Spring

This is an important phrase as it appears with "elastic" in 1822 and is implied for 1830. By the spring terminology seen so far, this could be a type of spring (perhaps the same as a helical spring?) or a part of a piston.

The list of X-patents has nothing for piston spring. The *Web of Science* had 43 hits, but nothing was relevant. Searching the history indices produced no hits.

Google Scholar was then used to do a search for "piston spring." The first was unrestricted in order to see any modern usage. For example, a pair of ca. 2000 patents turned up using a helical spring to act on a piston, resulting in an additional concept of "acts upon" for a spring descriptor. The date range was then restricted to 1800–1900, resulting in 53 hits, the earliest being in 1858. These were inspected, and the following were of interest.

US Patent 93,273, "Improvement in piston-springs," issued 3 August 1869, uses leaf-like springs to push a packing ring outward against the cylinder wall, pushing against some part of the piston. This gives yet another potential meaning for a spring descriptor, which corresponds to the modern definition of "piston spring" from the Merriam-Webster dictionary, namely "a spring for a piston ring." US Patent 171,157, "Improvement in modes of manufacturing piston-springs," issued 14 December 1875, is for springs of the same function, except they are helical springs. US Patent 501,560, issued 18 July 1893, is for a coiled trapezoidal cross section spring that is wrapped around the piston for forcing piston rings rings out, again, for the same purpose.

In a pair of patents from 1874 and 1898, a helical spring acts on a piston. So the "acts upon" concept is spanning more than a century of usage. There were numerous others of the same nature.

The search for nineteenth century uses of "piston spring" turns up two additional meanings, a spring that acts upon a piston and a spring that forces a piston ring outward. Significantly, there was no use of "piston" as a descriptor of spring shape. This means it is likely that use of a "piston spring" implies the presence of a piston, and unlikely that it was part of an accordion bellows, which has no piston.

For comparison with more modern usage, first consider [Kimball 1923]: in 1923 it appears that the term "helical spring" was in its current use, and there is no mention of a "piston spring." In 1967 *Mark's Handbook* [Baumeister 1967] on pp. 8-104–8-105 used the terms "cylindrical helical spring" and "conical helical spring" to differentiate these two types, but is not using the term "piston spring." Finally, the Wikipedia article on "Spring_(Device)" uses "helical," "coil," "spiral torsion," "flat," and other terms with "spring," and makes no mention of a "piston spring." So current usage seems consistent with period usage.

9.9. Elastic Piston

As stated above, the phrase "patent elastic piston spring" could possibly involve a contraption called an "elastic piston." This is a non-trivial question, since the patent titles from the indices make no mention of springs, just an "elastic piston." So the question then is whether such a device existed, and if so, what was it. Searching the history indices got no hits for this in the titles, while a topic search in *Web of Science* got only hits from 1957 to the present, which are not useful for assessing period usage. A Google Scholar search was done for “elastic piston” from 1800–1870. This got 8 hits, the earliest in 1843. The relevant ones are as follows.

US Patent 93,171, “Improvement in milk-can stoppers,” issued 3 August 1869, uses as a stopper a piston that is held tight against the milk by a helical spring. The elasticity is up and down, not outward. US Patent 3,186, issued 20 July 1843, has a steam engine piston in two pieces, the main body and the cap, with hemp packing in between for the piston ring. Instead of bolting the two pieces together to expand the packing, this is done by springs; this is the elasticity. US Patent 19,266, issued 2 February 1858, uses a piston as part of a spring mechanism to compensate a lamp against jerks; the elasticity in the piston is up and down motion. In US Patent 59,034, issued 23 October 1866, the “piston” is an elastic stopper that can be compressed between two nuts as it wears, to maintain it’s fit.

Outside of that search, Erie H. Oderman was issued US Patent 560,918, “Piston,” 26 May 1896. This was said by others to be an “elastic piston” and was referenced by US Patent 3,161,185, “Elastically connected pistons.” The piston was split into two parts along the piston rod, separated and held apart by a spring; the two parts compress during compression. No hint was given as to why. However, patent 3,161,185 says the point is to absorb shocks from fuel explosions, and to vary the volumetric ratio with the volume of gas admitted.

These examples shed additional light on the use of “elastic” at the time, as associated with a piston. This includes the earlier one where the piston is directly “springy,” but adds the more complex case where the piston is made of multiple pieces and uses springs to permit mutual movement. Finally, the term can mean simply that the “piston” can move in cases where it would normally be thought to be stationary.

Now the above must be considered against the patent title and plausible blast configurations. The most obvious part is that defining “elastic” as “moving” can be disregarded here: of course the piston in a blast is moving. Probably equally obvious is that the piston is not intrinsically “springy,” as, say, if it were made out of rubber. It is not a stopper and must achieve continuous smooth motion with minimal friction consistent with an air seal. An explanation at least plausibly consistent with one above definition, that of a multi-piece piston using springs, also seems unlikely. First, it would seem to have to be different from using piston rings. I have seen no use of the phrase “elastic piston” to describe rings, whereas pistons with rings and associated springs are clearly described as such. Plus, it is not clear how you make a circular “elastic” piston out of materials of the period, although a rectangular one seems tractable. But, none of the historical material contains any hint of such a device; where an air seal is required without the use of rings, some flexible material is used.

A further problem with the idea of an elastic piston is that the books describe installing “springs,” but the springs would only be a component of an elastic piston. This implies that the piston was already “elastic” and they merely installed an improved spring. The complexity would seem to be in the piston itself, not in the springs, so there seems to be a lot of effort over the years going into what would be a minor component, while there is no mention of the major overall device.

Back to the issue of the patent title. A plausible interpretation is that someone simply omitted the word “spring,” that it should have read “Bellows, elastic piston spring.” Perhaps it was an oversight, or maybe the indexer had not read the patent and thought that “spring” made no sense in the context.

A final issue is whether the piston spring may be “acting on” the piston rather than associated with piston rings. I cannot come up with a plausible scheme for this. A piston and cylinder blast is the only known device associated with the furnace that would have a piston, even if it is blowing something else, such as the cupola. There is no known place for a piston besides the obvious one, and there is no known reason for a spring to “act on” this piston, as it is driven directly by the piston rod.

In summary, it is concluded that the invention was merely a piston spring with a mistake in the patent index titles, and that the word “elastic” has no useful meaning. Further, the piston springs used in the blast were almost certainly associated with piston rings, as this is a known use, and no alternative use can be conceived.

9.10. Bellows Springs

Now we need to consider the meaning of “bellows spring,” as the phrase post-dates the use of “piston springs” in the book entries. None of the databases got any hits for this. A Google Scholar search for “bellows spring” for 1850–1900 got 11 hits, and none before 1850.

In US Patent 416,076, issued 26 November 1889, a spring presses on a leather accordion bellows, to compress air to force oil through a filter. In US Patent 521,258, issued 12 June 1894, a similar arrangement is used for a vacuum cleaner for chalk boards. In US Patent 302,523, issued 22 July 1884, a spring is used to re-open a bellows after it is closed by external means. A series of four US patents from 1868–1895 (numbers 80,167, 148,482, 355,985 and 546,956) all describe the use springs to re-open bellows in organs. This includes a V-spring inside an accordion bellows, and one includes the statement “in which case the ordinary organ-bellows spring may be used.” This implies that the use of springs with organ bellows was commonplace in the period. Also note that the patents were for broader devices, not for the application of a spring, further implying that such spring use was obvious.

Another source of period organ information is an 1852 book describing organ construction [Seidel 1852]. Pp. 39–40 states that both weights and a “bellows spring” are used to close the bellows after they are opened by the pusher. (It is possible that the spring is made of a long piece of wood in this case.)

All of these uses of “bellows spring” are for a spring that acts on an accordion bellows, either to compress or expand it. Of significance is the apparent common use of a spring to expand an organ bellows, which is approximately the same, except for size, as a wood and leather blast furnace bellows. However, while this is an obvious meaning for “bellows spring,” it is still the case that “bellows” is the same as “blast,” so there can be no assumption that a particular blast was involved.

9.11. Spring Summary

Here is a summary of what appear to be reasonable conclusions about the various aspects of springs with respect to Hopewell Furnace:

- A “piston spring” was not a type of spring.
- A “piston spring” was a spring acting on a piston ring, and an “elastic piston spring” was the same thing as a “piston spring.”
- There was no such thing as an “elastic piston” associated with the Hopewell blast. Only a conventional piston was involved, and the patent indices listing Dotterer’s invention simply dropped the word “spring(s).”
- A “bellows spring” can clearly mean a spring acting to expand or compress a leather and wood accordion bellows.

This permits one conclusion about the Hopewell blast, that it was almost certainty a piston and cylinder arrangement with piston rings by 1822. The “piston springs” of 1822 are too difficult to place with any other type of blast. (Note that this is not a claim that the 1822 blast was the same *in detail* as the current one.) The description of the 1830 springs is easily consistent with this, that they were “improved” piston springs, even though “piston” was not used while “pattent [sic]” was. The 1831 “bellows springs” is not so clear, but recall that “bellows” can describe any sort of blast.

I am taking this as the explanation due to the difficulty with dismissing piston springs and the extreme unlikelihood that they would have reverted the blast. There is finally the issue with "tubs" not appearing in the books until 1852. This can be reasonably dismissed as a change in terminology and/or an example of specificity. This could be due to a different clerk, or perhaps the repairs were to the tub walls or valves and the specific word was warranted.

The spring information alone *does not* permit any conclusion about the blast prior to 1822. The installation of Dotterer's springs could have been part of the construction of a new blast, or a replacement of inferior springs in an existing blast. So the spring information allows us to fix the general design of the blast with reasonable certainty for 1822 as similar to the current one, while not telling us anything about blast prior to 1822.

10. Leather Purchases and Finishing

The furnace books and KCFO document seven purchases of "bellows leather" in the historical period. These purchases need to be analyzed to see if they indicate anything about the blast design. These unqualified "bellows leather" purchases can be interpreted as being for a leather accordion furnace blast, the current blast, and probably any other type of blast that existed at Hopewell. As described below, and in Section 8, it appears that purchases for the blacksmith were identified as such, and this is not considered an option. The record of bellows leather purchases is shown in Table 2.

Year	Amount	Weight	Cost
1816			\$24.15
1826	Unspecified amount		
1827	Unspecified amount		
1831			\$16.24$\frac{1}{2}$
1835	2 Sides	52 lb	\$14.56
1849	Unspecified amount		
1850		35 lb	\$11.50
1877	SELL	5$\frac{1}{2}$ lb	\$2.75

Table 2. Summary of Entries for Bellows Leather Purchases.

The 1835 purchase added \$0.75 for finishing, and the 1850 purchase price includes finishing. Mention of leather buying ceases after 1850, but the sale is noted, and raises the question as to whether there was a lost purchase ca. 1870.

In order to know if a given purchase can or cannot meet some need, we need to know the area and thickness (or range of usable thicknesses) of leather required to fill said need. This is a problem since we do not necessarily know either of these for some uses, and clearly do not know either for any purchase. We do know the weights for some purchases, and when given a cost, can infer weights for some others. Based on the 1835 and 1850 purchases, the cost of leather for 1816 and 1831 is estimated at approximately \$0.25/lb. This gives estimated weights for 1816 and 1831. Nothing can be done with purchases that have no specification.

The relationship between thickness and area is density. According to `engineeringtoolbox.com` (and similar for some other sites), the density of dry leather is about 54 lb/ft^3. The "weight" of leather in modern terms, said to be "N ounce leather," is a measure of thickness, meaning N/64 in thick, and is approximately the weight of one square foot in that thickness. If the leather is 1/4 in thick (16 ounce leather), there is 0.021 ft^3 per square foot of hide, or each square foot weighs about 1.1 lb.

Given the stated or estimated weights and the density, the nominal area for each purchase can be estimated for various thicknesses. This ignores issues of fragmentation, whether it is possible to get the necessary leather in contiguous pieces out of a hide-shaped blank. We assume that the thickest leather used was 16 oz, being the thickest used for the blast restoration. 8 oz was the other thickness used. The range of areas as a function of thickness is shown in Table 3.

		Square Feet Leather			
Year	**Weight**	**4 oz**	**8 oz**	**12 oz**	**16 oz**
1816	100 lb	363	182	121	91
1831	70 lb	255	127	85	64
1835	52 lb	189	95	63	47
1850	35 lb	127	64	42	32

Table 3. Estimated Square Feet of Leather from Bellows Leather Purchases.

Now the available leather needs to be compared to potential uses. Prior to 1822 the potential uses are any sort of furnace blast. After 1822 the potential uses are the current blast or one similar to it.

The blacksmith bellows can be ruled out as a material consumer of leather described under “bellows leather.” As described in Section 8: entries associated with the blacksmith are seen to be identified as “smith;” the two later references were for buying complete bellows instead of building them; and even a complete blacksmith bellows requires only around 4 lb of leather. Further, bellows leather purchases apparently ceased after 1850, while Hopewell’s operation did not. There is no apparent reason why utilization of the blacksmith should be any different after 1850 than before. This lends credence to the claim that needs for the blacksmith are recorded separately from the activity described for bellows leather. There is still a question of the blacksmith bellows lifetime. Data from Section 8 implies that a unit from 1849 may not have lasted until 1883. However, merely one replacement in the middle would solve this, which could have been missed for several reasons. Alternatively, one missed purchase of just 4 lb of leather could also have rebuilt the bellows.

For a furnace blast, first consider the needs for a leather accordion bellows, for which a size must be estimated. Gordon on p. 106 states that a typical 1830s stack was about 30 ft high. Bining on p. 177 lists parameters for a number of 1700s furnaces, including Hopewell. The highest furnaces are 35 ft, also including Hopewell. The OHB gives the Hopewell stack height as 32.5 ft. My measurement in 2016 (and Long’s statement) is 30 ft. The reasons for the 5 ft range are not known, but this places Hopewell at the high end by height. Recall from the history section that leather bellows were characterized as up to 20 ft long by one author and up to 25 ft long by another. By the Catts/Cotter diagram of the north furnace room, the likely place for any ground-mounted blast, the space was only about 22 ft square. A few feet might be gained front to back if the noses of the bellows are inside the tuyere arch. Assume the 20 ft long units as the size of a “larger” unit commensurate with Hopewell’s stack size and consistent with the expected mounting space.

For this 20 ft long unit, assume it has a back end 3 ft high when fully inflated, and 5 ft wide at the back. Compute a leather area using simple triangles and ignore fragmentation problems. One side requires 30 ft^2, so one unit requires 75 ft^2 total, and two units are required. Assuming 16 oz leather, the 1816 purchase would suffice for only for one unit. For 8 oz leather, it is sufficient for both. So if a claim is made that the leather was for leather accordion bellows, the bellows size and leather thickness are critical parameters to determine. Note however that the McNeil comment about coating the leather implies that either thicker or thinner could be used.

The earlier Hopewell historians made a conjecture of what a leather accordion blast might have looked like. This is shown in a pair of drawings [NPS 1956]. These are scaled at $\frac{1}{4}$ in to the foot, and are lacking detail. They are showing a unit approximately 23 ft long and 9 ft at its widest point, with a circular back end, and open at the back to about 9 ft high. Note that it is unclear if these could have fit in the presumed space. There would have been no room behind them for the shaft and cams, but it seems possible that the shaft could have been overhead around the mid-point.

The diagram is showing the leather still partially folded, so take the leather height at the back to be about 25% greater, or about 11 ft. If the back is taken as an approximate half-circle of 14 ft perimeter and 11 ft height, it requires about 150 ft^2 of leather. Each side is a triangle of about 11 ft base and 18.5 ft height, or about 100 ft^2 per side. Thus each unit requires 350 ft^2 of leather, or 700 ft^2 total for both units. This implies 700 lb for 16 oz leather, 350 lb for 8 oz, and 175 lb for 4 oz. It would appear that none of the purchases could have been for building or releathering a blast this size. If such a blast existed, this is indicating that it was in prehistory, where the leather purchase records are not available.

For the current blast, we need about 32 lb for 16 oz leather and about 10 lb of 8 oz leather, for a total of 42 lb, ignoring fragmentation issues. The 1816 purchase would easily cover this. The 1831 and 1835 purchases are sufficient for the entire blast. Even the 1850 purchase could be sufficient if the leather was not as thick as called for in the restoration, and/or there was usable leather left over from an earlier purchase or already in the blast.

In summary, The 1816 purchase is potentially suitable for any blast, provided 8 oz leather is suitable, so the purchase is not telling us anything about the blast. The 1830's purchases are sufficient for the current blast. The 1850 purchase could do the same, assuming thinner and/or carry-over leather. If the current blast were already in place, but worn, the 1850 purchase could replace all of the 16 oz "wear" leather, assuming reuse of the existing 8 oz covering leather.

From a lifetime perspective, it is plausible that the 1830's purchases represented two partial rebuilds if the blast had been built in the 1810's, possibly with leather left over and/or used for the blacksmith. Then, 1850 would have been a plausible time for another partial rebuild. However, this last rebuild would have to last relatively longer, as no further leather was bought, or else a purchase was missed.

In summary, the 1816 purchase appears sufficient for rebuilding or building new any type of blast likely to have been used. And the later purchases are sufficient to build or rebuild the current or a current style blast. So the leather purchase data is consistent with other knowledge, but is not specific enough to constrain the blast beyond other knowledge.

10.1. Leather Finishing

There are two cases where "finishing" leather is part of buying it, raising the question of whether finishing tells us anything about the leather and hence blast design. Millen [Millen 2015] provided some background on leather finishing. This is the final step in processing leather, and substantially means to polish the leather and close the grain, partly using oils. It might include dyeing. One process might have been to pass it through rolls in other to get a more uniform thickness; this is said to compact the leather but not draw it out (as in blacksmithing). Typically this might be done at a tannery, but a tannery is said to have only produced leather for a specific purpose when the market for that purpose was large. This apparently explains the separate activity of finishing at the furnace, where unfinished leather was bought and finished specifically for suitability for the blast. Another part of finishing might be to set the pliability, which would be important for bellows.

It appears then that the "finishing" entries are not providing much information. Mainly they appear to indicate that the furnace wanted blast leather finished just so, so they did the work internally. The above processes apply equally well to any type of blast.

11. Dressing the Bellows

The phrase "dressing the bellows" appears frequently in the furnace books, and is a potentially important indicator of blast design, provided we know what it meant. As will be described, it likely meant general maintenance, and the dressing statistics change abruptly at two times. Provided the conclusions herein are deemed reliable, these changes are good evidence of non-trivial changes in the blast, changes that reduced wear and tear sufficiently to have reduced maintenance requirements. This subject is analyzed in this section.

The 1828 Webster's dictionary includes these phrases for the definition of dressing: "Adjusting to a line"; "putting in order"; and "preparing." These can be construed as general repair and maintenance. So the dressing entries could refer to the normal and non-specific work of keeping any blast in working order. (Another definition is "a flogging, or beating;" perhaps this is what they did to the blast when it was particularly uncooperative.) P. 53 of Boyer reproduces a letter dated 20 April 1787 in reference to Changewater Forge in New Jersey. It includes the sentence: "My Bellows will not want Dressing until October next." This indicates that "dressing" is not a term unique to Hopewell, and can refer to anticipated or regularly scheduled work, as opposed to only reactive repairs. That dressing was commonly known is indicated by an excerpt from Hopewell Document 8330601: "I do hereby certify that Robert Wright has Repaired the Bellows at this place for several years past, also has been employed at the different Iron Works in this neighborhood and his workmanship as a Bellows Maker & Dreser has been approved off by all that have employed in this neighborhood."

An exact definition of dressing is more difficult to find. Cranstone states that "'dressing' in the UK usually meant the repair of wood-and-leather accordion bellows." King [King 2015] states that "Dressing would [be] a work of maintenance, such as greasing the leather to ensure that it did not dry out and crack." Campbell [Campbell 2015] states that "dressing" can mean simple lubrication, and this is possible. However, routine lubrication should be simple enough that it was done by normal workers, and hence not warrant a book entry. Plus, as shown below, "dressing" was first erratic, and then ceased completely after 1850, while lubrication must have persisted (and does to this day). Another claim is that it means to apply neatsfoot oil to the leather on leather bellows. Recall that McNeil quotes a period source as stating that leather bellows must be oiled "continually." However, as shown below, the blast was usually dressed at most once a year, sometimes with several years between dressings. In 1832 and 1834, Robert Wright was paid for dressing the bellows twice. This indicates that dressing was a one-time event rather than continuous work that was only paid for every few years. An additional problem is that dressing persisted from 1784 (or earlier) to 1850 and not thereafter, while the blast was in its current style by 1822. This represents at least a quarter century of dressing something that was not a leather accordion bellows. So whatever dressing meant, it almost certainly was not oiling the leather in leather bellows.

Two book entries provide additional information. In both 1802 and 1826 there are separate entries for dressing and repair. This implies that they are different acts, and that dressing was more likely to be maintenance (i.e. due to normal wear) as opposed to the repair of sudden breakage.

In addition, on 1 February 1802, Samuel Cox was paid for one day of work "cleaning stuffing & oiling bellows". In this same year Evans was paid for dressing the bellows. Given the frequent references to dressing (and for multiple dressings at times), this is a suggestion that Cox's work was not normal dressing. The Cox work also implies that stuffing is different from oiling. That there seem to not be additional references to work like that of Cox gives uncertainty to how much meaning can be put into it however. In particular, the tasks seem simple and clearly do not require much time, so the tasks may be normally performed below the financial radar.

As a note, a historian notecard contains a list of items noted as "What a Bellows Dresser did" and references p. 31 of [Raistrick 1989]. This is basically an overhaul of a leather accordion bellows. However, there is no word like "dressing" anywhere in the description. Plus, this is implied to be the normal procedure before each blast over a course of about 60 years from 1690–1750. That dressing at Hopewell was erratic casts doubt that it was this procedure.

More insight into the data can be had by plotting it as shown in Table 5. The main series shows the dressing data. The dressing expenditures for the first several years are given in pounds; pounds were converted at $5 per pound for Table 5. (See p. 18 of Hermelin for comments on the value of money in the early 1780s.) The point labeled “bellows springs” reflects the entry indicating work on same. The two points labeled “piston springs” reflect the installation and/or replacement of same. The points labeled “tubs” represent the entries for tub repair and for straightening a piston rod.

The main observation is that the dressing data groups itself into three periods. The early period is through about 1840 and has greatly varying and (relatively) high expenses. The middle period, roughly 1840 to 1850, has much more consistent and lower expenses. This is unlikely to reflect a simple change in frequency as the average cost over the early period (dressing cost per year) is about three times that of the middle period. In addition, dressing is annual for parts of the early period, so there is no evidence that merely using more consistent dressing reduced the costs. The late period, after 1850, has no record of dressing.

There is a plausible but crude statistical basis for this three period division, using basic techniques from *Statistical Process Control* (SPC). (For an brief overview, see [Wheeler 1992] or [Deming 1982].) Note that since nothing certain is known about dressing, the application of SPC is shaky. Further, there is uncertainty about how to treat the years with no dressing expenditure. If dressing is mostly repair work, then these years count as 0. If they are mostly maintenance, then it is plausible that either the previous or next non-zero expenditure should be amortized over the adjacent 0 years. However, the latter case is only true if wear is linear in time, and better (i.e., more expensive) dressing results in a more robust blast that lasts longer; this seems unlikely. Lacking solid information on this, they will be treated as 0 for computing the mean and variance, but otherwise disregarded.

The early period has a mean of \$7.88 and a standard deviation of \$10.74, both per year, ignoring the 1784 data point. The middle period has a mean of \$2.68 and a standard deviation of \$2.28. The late period of course is 0. The idea is that if one period is an outlier in the “system” of another, then this supports the idea of treating them as separate “systems” and the implication that the blast changed between them. One test of an outlier is that it is 3σ away from the mean. By this test, all but two (non-zero) expenditures of the early period are outliers w.r.t. the middle period. No sigma test is possible in the other direction, as even 1σ away from the early mean is negative. However, another test is that 8 consecutive points are on the same side of the mean. This would make the middle period, collectively, an outlier in the early period. Since each of the early and middle periods can be construed as an outlier in the other, support is given to the idea of a blast modification. The 8-point test says the same regarding the middle and late periods, that each might be considered an outlier in the other, and hence the idea of another blast modification.

The first conclusion is that this data is *not consistent* with the idea that the blast in question belongs to the blacksmith. Besides a few periods, the furnace was operating for the entire time shown on the chart, so blacksmith services should have been relatively constant. Molding stopped in 1844, but it is not clear why molders should require enough blacksmith services to significantly change the amount of maintenance required on the forge. Plus, molding stopped in the middle of the middle period, while dressing continued. So the dressing is clearly for the furnace blast.

The second conclusion is that the data supports the idea of non-trivial blast changes or conversions at the inter-period transitions, and does not support the idea of same at other times. The first clause is more likely to be real; it seems necessary for some change to have been made to the blast in order to get the significant changes in periodic work. While these are consistent with the possibility of a major conversion, they do not imply same. The second clause is not as firm. Assuming that “dressing” applies to non-specific work, it is possible that a new or significantly changed blast is just as troublesome, but in different ways, as the previous one. The nature of the dressing work might change, but coincidentally, without a change in the cost or frequency. In particular, the idea of a conversion *in* 1822 is not supported, but not ruled out.

A third conclusion is that the springs, either bellows or piston, whatever they are, do not affect the blast as seen through the dressing statistics. This is consistent with the idea that the blast is unchanged over the entire early period, and the spring work merely represents replacement of some component. However, this does not rule out the possibility of a major change, including the conversion from leather bellows to wooden blowing tubs, that by coincidence does not change the cost and frequency of dressing.

In summary, the phrase “dressing the bellows” very likely means maintenance of the blast, and is not specific to any particular blast design. It also seems to refer to repair of wear as opposed to sudden breakage, although this is less certain. Given this, the two changes in dressing statistics are likely indicators of some change in blast design or details that reduced wear. This may or may not be or include wear of leather components, although leather is more susceptible to wear than wood and metal.

11.1. Dressing, Springs and Leather Purchases

The question is whether there is a plausible description of blast changes that is consistent with the spring analysis, dressing data and other information, without requiring coincidental changes and/or non-changes in the dressing data. This can be partially addressed using some assumptions based on correlations with leather purchases. Dressing does not necessarily have anything to do with leather. However, leather is probably present in all the blasts and is likely to be a substantial wear item compared to wood and metal. So “maintenance and repair” could well be substantially “leather work,” and “dressing” could be correlated to leather, regardless of blast type.

The dressing data is suggesting non-trivial modifications to the blast at the two inter-period transitions, while the spring analysis argues against a leather to tub conversion at either point. The 1830’s and 1850 leather purchases are consistent with rebuilds of the blast. Most or all of the leather might have been replaced, with possible repairs and/or improvements to the remainder of the equipment. One possibility is the new leather was in some way better than the previous, and suffered less wear, although I do not know whether this is plausible. A more likely explanation is that the work included an improvement or replacement of components of the blast so as to decrease wear in general and on the leather parts in particular. For example, an improvement in the crossheads might have kept the piston motion better aligned with the cylinder axis, reducing wear on the piston leather. Perhaps the cylinders were refinished to make them rounder and smoother. By this logic, the 1831 leather went into simple replacement, while the 1835 leather was associated with both replacement and further blast improvement that gave rise to the middle period dressing statistics. Then the 1850 leather work would be similar to 1835, replacement and further improvement, eliminating the need for the periodic notable work.

The 1816 leather purchase does not provide any information regarding the blast, even considering the lack of change in dressing statistics. It is known that the furnace had been idle for around eight years and that significant money was spent putting it into operation; it is a reasonable possibility that all the leather needed replacing. The leather purchase was potentially sufficient for any type of blast, and could have been used to re-leather the existing one or build a new one.

11.2. Blast Conversion?

The dressing data leaves unresolved the question of when (if ever) a leather blast was converted to a cylinder blast. The explanation most directly suggested by the dressing data and spring history is that the blast never changed during the historical period, that it was already a cylinder blast by 1800. In this case, only springs were changed prior to the 1830’s, and the 1816 leather purchase was to replace leather neglected during the shutdown. The alternative explanation is that the conversion was done in the early period, most likely in 1816, the only time prior to 1822 with a substantial leather purchase and millwork. This requires that the dressing needs were, *by coincidence*, the same before and after. Note that this assumption does not indicate what type of blast came before. So while the dressing data is suggestive of a conclusion, it is not certain.

12. Interpretation of Terminology

Some of the book entries contain terms that are unknown or whose meaning is unclear, meaning that we do not know (for certain) what they were talking about. The documents do not explain them, since *they* surely knew what they were talking about. Putting a meaning on these terms is potentially productive, since knowing exactly what a device or procedure was may (but may not) constrain the design of the blast it was associated with. Note that for this to work we need to determine the meaning of a term with minimal context, so that it *provides* context for determining the blast. Figuring out what a stirrup is given the blast would be nice, but the objective is to go the other way, to figure out what a stirrup might have been, in and of itself, and deduce blast characteristics from that.

The following subsections review what has been found for the lesser terms in question. Unfortunately, this has not been a productive exercise.

12.1. Stirrups

On 27 September 1832 Samuel Knaur was paid $0.80 for sawing "200 feet Stirrup Stuff". This was in a block of payments for various sawing, including lath, so this is clearly wood. On 20 May 1833 Robert Wright was paid $23.00 for "Dressing the Bellows twice & putting in new stirrups in 1832". Given other dressing costs in the period, most of the $23 should be allocated to dressing, leaving only a few dollars for stirrup replacement. The entry does not specify whether this payment includes the cost of the stirrup. In addition, Martha Furnace (NJ) also used stirrups and apparently had a blast similar to what is presumed at Hopewell for 1832. In Pierce on p. 103 there is a section of "The Martha Furnace Diary." The entry for 26 April 1810 includes the item "Bellows stirrup broke." On p. 88 Pierce refers to "the great pair of bellows" and then states that "Martha used bellows of the tub type."

The Webster's 1828 dictionary contains only the implement used on horses, and [Gwilt 1867] does not have it in the glossary. Campbell says that in construction, including in 1800's terminology, a stirrup can be a U-shaped metal tie used to connect structural members; a beam could rest in a stirrup. Kricker [Kricker 2015] does not recall hearing of a stirrup associated with a furnace, but says that in a reciprocating-type saw mill, it is the fitting that holds the blade. Zink [Zink 2015] believes this would be a metal fitting attached to a moving part. Cohen [Cohen 2015] has not heard of its use with a structural element, but sees it as possible. Cranstone states "I'm sure a 'stirrup' is a component of a steam engine or other cylinder engine though I can't find a definition or illustration to confirm that." King states "The stirrup might be linking different parts of the mechanism together." In his description of leather accordion bellows, Agricola does not show anything labeled as a "stirrup," but part "V" in the diagram, the "staple," might be construed as stirrup-shaped. The staple was bolted to the bellows bottom board, and the ring to the supporting sill, to keep the bottom board stationary.

There is little information here. If a stirrup describes any metal fitting attached to a moving part, various components of many blasts might qualify. If it is a structural tie, or something made out of wood, any blast could use them.

12.2. Paicers

In the spring of 1833 Robert Wright was "Putting up the Bellows & dressing them, Putting up Paicers etc" for which he was paid $70 on 6 June. The breakdown is not given; it includes one dressing, to which might be allocated $10–$25, giving a significant sum for the remaining work. On 9 April 1834 Isaachar Pawling was paid $14.00 for "Making Sundry iron for the Bellows & Pacers" sometime between 19 June 1832 and 5 April 1834 (SM 21 p. 99B). It seems likely that these two entries are related. Taken together, they indicate some non-trivial work on the blast in the spring of 1833, that paicers were associated with the blast, and that paicers were some sort of construction rather than a stand-alone item.

My contacts are not familiar with the term, and I have never seen it except in these two entries. It is not in Websters. An unlikely possibility is suggested on p. 1311 (Glossary Addendum) of Gwilt. This defines the word "Pace" as "A portion of a floor slightly raised above the general level ..." Perhaps it refers to part of some platform, and there is a variation in spelling. An interesting spelling question arises from p. 3 of a 1948 Kurjack report [Kurjack 1948], where he raises the issue of several terms, including "pacers." I assume it more likely that he dropped the "i" in the report rather than added it twice in KCFO. However, even if this is the case, it is not telling us anything about the blast.

Paicers remain a partial mystery, and what is known does not say anything about the blast.

12.3. Trunk

There are three book entries concerning a "trunk," for 1834/1835, 1848 and 1878, but no mention as to what a trunk was. There was no mention of a trunk in any of the historical literature on blasts.

There is a set of three payments that seem to be associated with the same work in July–August 1834. On 13 August 1834 Robert Wright was paid \$12.69 for $14\frac{1}{2}$ days "work at the new trunk" and other partially unspecified things. The cost of the trunk is not clear, but the weight may be $23\frac{1}{2}$ lb at \$19.89 (KCFO). There was also much work associated with wheel in the same period. On 2 June 1835 Elisha R. Sands was paid \$7.59$\frac{1}{2}$ for $6\frac{3}{4}$ days "work done at the furnace trough from the 30th of July to the 9th of August 1834". Since Wright was paid on 13 August, these payments seem to go together. Finally, on 1 January 1835 George North was paid \$7.20 for "9 days work at the new trunk". This is part of a block of payments from a time book for the period "1st August last" to 1 January 1835. Thus it also appears to be part of the same work done by Wright and North.

In 30 January 1849, Isaac Markley was paid \$44.00 "for work done in putting up a new trunk & dressing bellows in May 1848" (SM 32 p. 59A). The payment is broken down by worker but not by function. In this Isaac Markley does no work, while these men are paid for: Peter Markley, 11 days; George Griffeth, $9\frac{3}{4}$ days; Isaac Boyer, 9 days; David Dunlop, $5\frac{3}{4}$ days; and David Jrie(?), $5\frac{3}{4}$ days. Assuming that Peter is doing the dressing (being related to Isaac, who is known for dressing), he presumably did not spend a lot of time doing it, as he was likely matching the days done by others. At his rate of \$1.25/day, this indicates 2-3 days for dressing, hence the \$3 estimated earlier. So this was around 39 man-days of work, probably done over a week and a half of calendar time.

On 1 January 1879, payment was made for "work at water trunk", \$6.30 for 7 man-days total: John Care, 2 days; Nathan Care Jr., 2 days; and H. A. Long, 3 days.

The term "trunk" is mentioned in Seidel's book on organ construction. On p. 38 the "wind box (or wind-trunk)" opens into the "great trunk" through valves ("wards"). On pp. 40–41: "The wind emanating from this latter pair immediately fills the trunk and all the other conductors of wind." Hence the trunk appears to be the main wind pipe. This is plausible for a furnace blast, but seems odd when there is only one tuyere. In a modern furnace there is a bustle pipe, which distributes air from the blowing engines to multiple tuyeres, so this might be called a trunk. This seems out of place at Hopewell however. Other KCFO entries also make direct reference to a blast pipe, so using an alternate term seems odd.

Webster's 1828 dictionary includes these contemporary definitions: "The main body of any thing" and "In architecture, the fust or shaft of a column". Gwilt on p. 1272 also includes the second Webster definition. By this, it could mean a significant part of the structure that holds the blast. Cohen has not heard of the term used as part of a structure.

Campbell describes a “trunk” as some sort of trough or duct that carries something. This could apply to the blast pipe, and also to the headrace flume. A second Gwilt definition is “a vessel open at each end for the discharge of water, rain, &c.” This also describes the headrace, and, at a stretch, the tail race. I do not recall any mention of “trunk” in association with the headrace in any literature. However, I also do not recall the use of “flume” in any Hopewell literature. The archeological descriptions of the west headrace make it clear that there was a flume in the historical period. Using “trunk” for the tail race seems unlikely. The tail race already has a name, and I have never seen it called anything else. Plus, the trunk is described as something that can be purchased and “put up,” whereas a tail race is primarily just dug.

The payments for the 1834 work appear to be significant. The work dates for Wright at the “trunk” and Sands at the “trough” appear to line up, with the North work at the “trunk” probably for the same interval. This makes for a plausible relationship between the trunk and the trough. They are possibly the same thing, but this is not clear. They could be different, with the trunk being the east headrace pipe and the trough being the final east headrace conveyance from the pipe to the wheel. There has also been mention of headrace water being routed into the cast house for cooling tools, so this could account for a trough. Note that the 3 payments are about half a year apart each, so it is plausible that the clerk used a different term (“trough”) for one of them even though it refers to the same piece. In the latter case this implies that a “trunk” and “trough” are the same thing, and the same as a wood flume. Note that blast data indicate that the furnace was running continuously over July and August of 1834, raising a caution about claimed work on a headrace. However, since the current (west) headrace is presumed to have been in operation by 1834, and the power computations indicate that the east headrace supplied no more than a third of the water, it is plausible that the east headrace was shut down for the work, with the furnace running at reduced output.

My first conclusion is that the trunk was associated with water and the wheel. This is mostly the fact that this would be consistent with other sources about the term, the use of the explicit phrase “water trunk” for one instance of work, and the plausible association of the word “trough” with another instance. The two other entries say only “trunk,” but this would imply that there was only one trunk, and the clerk felt no need to further identify it in the two cases. Second, the trunk probably was part of one of the head races. The 39 man-days by 5 men in 1848 makes it clear that this is a significant structure rather than some isolated object that can be dropped into place. Similar for the 30 man-days by 3 men in 1834.

Most likely, the trunk refers to the elevated west race flume (possibly including it’s piers), a final trough or flume for the east headrace, or the buried pipe for the east headrace. Regardless of the details, the apparent association of the trunk with the wheel means that it is not part of the blast proper, and is not helpful for identifying the blast design.

12.4. Piston Stem

An 1840 book entry includes mention of a “piston stem.” This is not in Gwilt, and Websters only mentions horticulture, genealogy and part of a ship. On pp. 284–285 Agricola describes the construction of a stamp mill, and a “stamp-stem” is clearly shown as a square rod that is moved up and down with the stamp head at one end. This is consistent with the interpretation that a “piston stem” is the same thing as a “piston rod.” A more general “stem” might be interpreted as any rod that pushes something, possibly an actuator between the water wheel and any sort of blast, including a leather accordion bellows. However, inclusion of the word “piston” implies that a piston and cylinder were involved.

Given the conclusion from the spring analysis that a cylinder blast existed by 1822, the assumption that this is a piston rod might be taken as given, or that it is one of the two rods associated with a crosshead system.

12.5. Stuffing

In 1801, wool for stuffing the bellows was bought, and on 1 February 1802, Samuel Cox was paid for one day of work in "cleaning stuffing & oiling bellows". These are the only two entries known that refer to stuffing, and represent only a single instance of doing this work over the furnace lifetime.

One obvious meaning of "stuffing" is the material used to pack the stuffing box that seals the penetration where a piston rod enters an enclosed cylinder. Note however that a single-acting cylinder has no need for stuffing since one end can be open. Cranstone states that "A stuffing box was placed on the top of a closed-top cylinder where the piston rod passed through it, to provide a seal - though I think the word might also be used for the packing on the edge of the piston to provide a seal against the cylinder." King states that "Stuffing might be the equivalent of caulking a boat - filling any holes from which air was leaking." The use for leather accordion bellows is not obvious, unless associated with the valves.

However, [Apple 1956b] on P. II-121 states that stuffing means to coat and treat leather to make it airtight and waterproof. This is difficult, since the only known reference to "stuffing the bellows" is in 1802. As stated, a leather furnace bellows was said to need "continual" treatment with oil to keep the leather airtight and/or prevent it from cracking. If stuffing was such an oil treatment, it should show up much more than once per century. In addition, the separate enumeration of "stuffing" and "oiling" in the Cox work suggests that these were different actions. Also note that all three of these actions could apply to almost any type of blast.

Daff on p. 401 states that for leather accordion bellows, "The nose of the bellows, where they joined the blast pipe, was packed with moss or wool." The 1828 Webster's definition of "stuffing" includes "That which is used for filling any thing". This is consistent with the 1801 entry being for wool for air piping for a leather bellows. This does not necessarily imply a leather blast however, as the wool could have been used to seal any type of joint in the air piping for any type of blast. Overman on p. 411 makes reference to a stuffing box for air pipe joints, a period reference to similar terminology.

So these entries are not providing any information on the blast design in 1802.

12.6. Camting

On 13 August 1834, Robert Wright was paid \$12.69 for $14\frac{1}{2}$ days "work at the new trunk & makeing Cam?ing pattern etc etc by himself & Anderson". Note however that there is a line break at the question mark, so one line ends with "cam". Based on other hand writing, the most likely interpretation is that they made a "cam ring pattern". However, KCFO describes this as "new trunk, camting (?) etc." Despite the probability that this is "cam ring" or possibly "casting," a historian saw "camting" and it would be significant if an obscure part were used, as this could reflect back on the blast design.

None of the historical literature made any mention of camting. A Google search for "bellows camting" turned up a hit for the 24 June 1863 edition of the Daily Ohio State journal (Columbus, Ohio), in the phrase "Malleable Iron Camtings." The OCR is not good and an OCR error appears to be responsible for the hit. Zooming in to the page image shows an ad for the "JAMES L. HAVEN & CO." of "Cincinnati O." with "Price lists mailed to all, free of charge". The OCR output was not well organized, but "camting" appeared only once and in the vicinity were items such as "BLACKSMITHS' BELLOWS," "Howell's Patent Trip Hammer" and "Iron Castings". Inspecting the image instead shows "Malleable Iron Castings" in between "Iron Castings" and "Howell's Patent Trip Hammer". The ad immediately above it has "BLACKSMITHS' BELLOWS".

No other search found a camting, so it appears that the KCFO entry was a mis-reading, and what was made was a pattern.

12.7. Summary of Terminology

Unfortunately, none of the terms described above were able to add anything to knowledge of the blast. No written material was found describing their meaning with regard to a furnace or blast, and the people consulted, collectively, did not have conclusive views on meaning. The latter lack of information offers a bit of information, that the meaning is not clear. In particular, it indicates that the period understanding has not carried through to the present. Finally, the plausible meanings for most terms cover enough of a range that they could apply to a variety of blasts, so the hope of tracing meaning to blast type has failed.

13. Discussion of Millwright and Other Work

There are various book entries related to work on the wheel and blast. These are analyzed for what they indicate about blast design. The list of entries is shown in Table 4. Entries for "work at" are classified here as repair, even though they might be maintenance. The abbreviation "MD" stands for man-days.

13.1. Wheel Repair and Replacement

Water wheels rot over time, and need periodic replacement. The first thing to do is establish the replacement pattern in this data. Not all the replacements are directly recorded, but there appears to be a consistent data pattern. An estimate of wheel life under Hopewell's conditions can be made from modern times. The original restored wheel was finished in 1952. Replacements were made in 1978, 1988 and 2006. This averages 18 years/wheel.

There are only two explicit statements of wheel replacement in the historical period. The first is for 1830–1831, when they recorded first a new shaft and then a new wheel, for a total of 84 man-days of work. There is a record in 1834 for work after the shaft broke, but this is unlikely to be an entirely new wheel. It is presumed that the 27 man-days under "new wheel" was to build the replacement parts, while the 30 man-days under "repair" was to install them. The other direct statement for a new wheel is Yocum's reference to Long about a new wheel in 1879.

It is likely that a new wheel was built in 1816, although this is not known. Sufficient time is certainly documented: using the 1830–1831 data of 84 man-days for a new wheel shows that the 1816 work could have easily included a new wheel. This gives an approximate 15 year life to the 1830 replacement, consistent with expectations. This would place the prior replacement before 1800, in the pre-historic period, consistent with the lack of replacement records from 1800–1816. Also note the frequent wheel repair during the historical period prior to 1816, consistent with a lack of a new wheel.

There is a 48 year span from 1831 to 1879, which would be expected to require three wheels, two after the 1830–1831 one. The second of these two could plausibly have been installed in 1867, due to the breakage recorded in late December 1866. This probably leaves one new wheel undocumented, estimated to be in the 1840s, although possibly in 1852–1853. While the data is clearly not complete, it is consistent with periodic replacements approximately every decade and a half.

The data can also be compared to prior report statements about the wheel conversion. The wheel conversion is assumed to have involved a wheel replacement, since the size apparently changed. The data is not consistent with a wheel replacement in the early 1800s: the frequent wheel repairs without any mention of a new wheel implies an effort to keep an old wheel going, not replace it. Plus, one would expect the repairs to stop once a new wheel is installed. So the millwright data indicates that the replacement of the north-south wheel with the current design happened either pre-1800 or else in 1816 or 1830–1831, but not in the period 1800–1815.

Year	**Repair Wheel**	**New Wheel**	**Millwright**	**Repair Blast**
1785	6 MD/£2.2.1$\frac{1}{2}$			
1801	£0.18.9			
1802	30 MD			£2.12.6
1804				1 MD/£0.7.6
1805	10 MD			
1806	£2.10.0			£2.17.6
1807	Part £4.10.3			Part £4.10.3
1816			237 MD/$304	
1824	2 MD			
1825				$12.00
1826				$9
1826			46 MD	
1830		6 MD	12 MD	Est 10 MD/$10
1831		78 MD/$59		
1832				New Stirrups
1833				Work, Paicers/$60
1834	30 MD	27 MD		New Trunk
1838			$288	
1840				Screw for Piston Stem
1845				Repair furnace pipes
1848				New Trunk
1849			25 MD	
1850				$3 repairs; Tin blast pipe
1851				Copper pipe
1852				2 MD repairing tubs
1853			77 MD	$1/Boot for blast pipe
1854				Repair Cylinders/$13
1866	Wheel broke			
1869				Straightening piston rod
1873				Repair Tubs/$11
1874				9 MD Repairing Bellows
1876				Turning wrist; crank casting
1878				7 MD/$6, work at water trunk
1879		New wheel		
1880				$5.50

Table 4. Table of Millwright and Related Work.

13.2. 1816 Work

There is a particular issue with the work claimed for 1816. The furnace had been out of blast since 1808, and we have the letter quoted earlier about significant work being required to put it back into operation, with a claim that $8,000 was spent. Given that this figure would support a huge amount of work, what was done needs to be analyzed.

First, the $8,000 figure is disputed. This was probably supposed to be only $800, and even this seems to be a nice round number instead of a figure from the books. Recall that Overman stated that a complete simple single-acting wood cylinder blast cost $500-600; claiming a need to spend 15 times that just for repair seems absurd. Further, consider the millwright figure of about $300. Based on book details, this was done over a span of 2$\frac{1}{2}$ months. So spending just $3,000 would have kept four millwright-quality craftsmen continuously busy for over two years, and then that amount must have been spent twice again, all in less than a year. Finally, furnace books (day books and main journals) are available from 20 March 1816 to past the end of 1817, and were inspected as for all the relevant books. Only $579 worth of furnace work was found for 1816. My conjecture is that the iron master merely estimated the figure, perhaps included repair costs for unrelated structures, and possibly wanted to make it sound impressive. Then somehow a zero got added.

The nature of the millwright work is not specified. As stated above, a new wheel is conjectured for 1816 independently of the availability of millwright time to do it. This would have taken about a third of the millwright resources, leaving around 150 man-days and $200. Recalling Overman's cost estimate for a blast, this is only about a third of the cost for a new one, indicating repair only. Assuming the blast was idle and not getting lubricated, it is reasonable that normally greasy areas would have dried and/or rotted, requiring disassembly and reassembly. So it seems plausible that refurbishing a neglected blast might have cost a third of the amount to build a new one. Finally, the entries for the millwright work paid the men by debiting the "Repairs" account. The general ledger for this period is missing, so it is difficult to know exactly what this meant at the time, but there is a clear implication that this work was for repairing existing equipment rather than building a new blast.

The final item, which may not have anything to do with the furnace, is $87 spent on 262 perches of wall. This is of concern since it in theory could have involved work for a new wheel pit, and hence be indicating a wheel conversion. Pp. 11–15 of [Apple 1956b] discusses the meaning of Hopewell's historical perch, and it appears most likely that it meant $1\frac{1}{3}\mathrm{ft}^3$ of wall volume. This implies about $350\mathrm{ft}^3$ of wall, and since many of the retaining walls are at least 2 ft thick, about $175\mathrm{ft}^2$ of wall area. The archeology data is indicating walls around 9 ft high for the current wheel pit. This would be sufficient for less than 20 lineal feet of wheel pit wall, a fraction of what would have been required. Whatever the work was, it was not to build the current wheel pit, and may not have even been associated with the furnace area.

In summary, it is concluded that the work done in 1816 was for repair and refurbishment of existing equipment and building a new wheel, and not for a blast or wheel conversion.

13.3. Millwright Work

The millwright work is *unspecified*. This is not directly tied to work on the blast or the wheel, but presumably could be associated with either, or something different, and in all cases, split among different jobs. The 1816 work was dealt with above.

Next consider the 1838 work, for $288. It is unlikely to include a new wheel, as this is only 7 years after a new wheel was installed and 4 years after the shaft was replaced. Recall the Overman statement that a single-acting cylinder blast without crossheads cost $500–600 when made out of wood. Hopewell's current blast is more complex than this, so the $288 total expenditure is clearly inadequate to build a new blast of a plausible design. Note that 1838 is in the inter-period between the early and middle dressing periods, where the dressing needs suddenly drop significantly. So this would be consistent with significant improvements in detail to the blast that enabled it to run with significantly less wear. But this requires a similar-style blast to start with, one that can be merely modified, as there is not enough money to build a new one.

Also note that claimed purchase of "piston stems" in 1838 and the out-of-books payment for forging "piston rods" in 1839. (The latter payment could have been between "subcontractors" for the more general work.) It is plausible that stems and rods are components of the overall piston connecting mechanism. This suggests one possibility, that a single-acting blast was made double acting with the addition of crossheads, which would require acquisition of a new means to connect the crank and piston, now in two pieces instead of only one.

There are two lesser entries ca. 1850: 25 man-days of millwright work in 1849 and 77 in 1853. This is in the middle to late period transition for the dressing data and is consistent with the idea that some improvements in detail were made to the blast at this time. Note that the middle period already requires relatively little dressing, so there is no conceptual need for a large amount of work to reduce this to nothing.

13.4. Blast Work

Kurjack claimed that difficulty with the blast in the early 1800s lead to the need for the stamping mill ([Kurjack 1949a], p. 1), and seems to use this claim as a basis for concluding that a Coventry blast existed. However, this claim is not supported by the data. For the entire historical period up to 1808, there are four instances of blast repair, one for one man-day, and the other three for less than £3 each. This does not represent significant repair over about eight years of service. (The wheel however requires repairs in five out of these eight years, and significantly more work. Perhaps this is what Kurjack was looking at.)

It is possible that a sufficiently wretched wheel was turning slowly enough to result in an insufficient blast. This is reinforced by observing the lack of wheel repair entries for the remainder of the furnace life. This implies that a proper wheel needed only routine maintenance and did not break, casting doubt on the quality and/or age of the pre-1808 wheel. So while this does not inform blast design, it suggests that the existence of the stamping mill may not be an indication of the blast design.

Note however that the production data in the next section casts doubt on the idea of any intrinsic defect in the furnace that would prevent good production, leaving uncertainty as to the reason for seeing the stamping mill at this time, but not later.

The entries in the "repair blast" column do not appear to have any significance; they seem to represent routine repair and replacement of parts, and the quantity does not seem great over three quarters of a century of service.

14. Comparison of Conjectures to Production Data

As noted in the introduction, production data cannot be used to indicate blast design changes, because additional variables influence production and capacity. However, it is important to see that conjectured blast changes are not contradicted by data, and to see whether data supports any previously conjectured changes that are being discounted in this work. This section examines production data gathered for other work.

The basic data is shown in Table 6. This shows all blast periods for which duration and total production are known or reliably estimated. Production is in average long tons per day over the entire blast period. Also shown are the tenures of the founders. Almost all the data is taken from furnace books, with a few data points taken from KCFO. Some of the data is estimated from related furnace book entries rather than being found explicitly, but the estimates are generally tight, and should have little effect on the average daily production.

14.1. Current Conjectures

Blast stability on 1800–1838: The most obvious observation is a general upward trend over this period, with wide variation. This does not contradict a stable blast, since for variation to result mainly from blast changes would require a frequently changing blast to account for the trend, with no explanation for the variation. Founder variation is also a plausible explanation for the data. There is no discernible trend in the pre-1820 data alone, which has frequent changes in founder (ignoring the 1808-1816 shutdown). The lengthy tenure of a single founder (Thomas Care) might explain the general trend starting after 1820. The point of the founder observations is that there is no need to find a blast change in order to explain the production data in this period.

The two pre-1810 outliers are further suggestions of a stable blast. The 1800 blast at 2.7 T/D (tons per day) is fairly reliable, with the production and end date known explicitly and the start date well estimated. The two 1806 blasts have complete data given, and production of 2.2 and 3.5 T/D. The latter is the highest overall production for the operating life, and raises the question as to whether the clerk transferred some of the production from the first blast to the second, since there were only about three weeks of gap between the two. However, even if the two are combined, the overall average is 2.7 T/D, the same as for 1800. The 2.7 figure is higher than any other blast average in the 1800–1838 interval. This makes it impossible for a blast change in the middle of the interval to be a requirement for the production seen at the end of the interval.

Similar reasoning can be applied to the production for 1783–1784, of 750.1 tons. The problem is that there is no explicit start or end date for the blast period, although the blast must have started in 1783 and ended by the start of April 1874, based on the founderage payment on 8 April 1784 (SM 41, p. 29). There is a block of SM 41 entries for paying furnace workers in December 1784, but unfortunately, there is another blast in the middle of 1784, so it is impossible to allocate the worker intervals between the two blasts. If the longest single payment, to Edward Hughs for 8 months and 6 days, is allocated entirely to the 1783-84 blast, the average production is 3 T/D, again higher than all the figures for 1816–1838.

An estimate of average daily production for 1783–1784 can be derived by taking a sorted list of all known blast durations as a crude cumulative distribution function of blast duration. There are 77 suitable blasts, so the first 69 constitute the first 90% of the durations. The 69^{th} duration in the list is 12 months. This approximates a 0.9 probability that the blast period is no longer than that. 365 days represents a production of 2.1 T/D. This is taken as a high probability that ca. 1784 the furnace production was in the middle of the 1800–1838 range, which discounts the possibility that the blast was significantly inferior to one later in the period.

A final piece of evidence for prehistoric production capability is in a letter to a Hopewell manager that is quoted by Walker on p. 232. This states that in proper operation the furnace should give at least 20 tons/week, in the 1790's. This rounds to 2.9 tons/day. Also note that Robbins on pp. 55-56 states (although without references) that the two Principio Company furnaces operating in 1736 were also capable of producing 20 tons/week of pig iron, half a century before the Hopewell letter, indicating that the letter writer is not making an unreasonable claim. Also, Schubert on p. 349 has a table that includes daily production for various English furnaces in the first three quarters of the 1600s, and shows several at 2-2.8 tons/day, and one at 3 tons/day. This is a century prior to the building of Hopewell, further evidence that 3 tons/day in the 1770's is not unreasonable.

In summary, the evidence is that the furnace was capable of as high a production in the late 1700's as in the period through 1838, so there is no evidence to support the idea that a blast replacement was required in order to justify the production level late in that period. This does not rule out a blast change, but only demonstrates that the data is consistent with the claim of stability over 1800–1838.

Conjectured 1838 blast change: There is clearly no abrupt change in production around 1838, although the general trend of increasing production continues. This does not contradict the conjecture, and there is no reason to expect that a change in blast would have necessarily increased production capacity; it could have merely improved reliability.

Conjectured minor blast change ca. 1850: Like the 1838 case, there is no abrupt change, but a continuity in variation, and a continuation of a general downward trend. Again, the conjecture is not contradicted, and there is no necessity for an increase in capacity.

14.2. Prior Conjectures

A number of blast change conjectures by prior Hopewell historians are being discounted by this work, and the production data is not supporting any of these.

Conversion to a Coventry Blast prior to 1822: Since the data is consistent with a stable blast in this period, it is not supporting this change.

More specific conjectures of conversions in 1816, 1818 and 1822: As before, consistency with stability means no support for a change for any of these years.

Conversion ca. 1851: The production data has started a general downward trend, which does not support a blast conversion, which presumably would not have been intended to compromise performance.

14.3. Summary

In summary, nothing in the production data is contradicting any blast change conjecture made in this work, nor supporting any of the prior conjectures that are being discounted. It is important to note that there is no need for a new or modified blast to have a higher discharge. Increasing reliability and reducing maintenance are fine reasons for an upgrade, so the lack of clear support for a blast change does not contradict a conjecture that a change occurred.

15. Discussion of Period Blasts

This relates information about what is known about other furnace and forge blasts in Hopewell's area and time, aside from the leather accordion bellows. This may help inform conclusions about what Hopewell was using.

15.1. British Practice

The state of the art in Britain is a consideration when estimating what was done in His Most Excellent Majestie's Colonies and Plantations in America, since commerce implies that knowledge was likely to be flowing.

The state of the art in the first half of the eighteenth century is indicated by Diderot. Plate 87 of the Dover reprint shows double leather accordion bellows with lever-arm counterweights used for re-opening [Gillespie 1959]. Volume 1 was published in 1751, so this probably represents modern practice in the 1740s.

Cranstone describes the state of furnaces in Britain at the time: "In Britain by 1771, a new furnace would normally be powered by blowing cylinders - two or four open-topped and single-acting cast-iron cylinders, each containing a piston operated by a lever-and-counterweight system from cams on the waterwheel axle ..." He notes that new furnaces in Britain were coke instead of charcoal, which required a greater blast, and this contributed to the use of cylinder blasts. Conversion of *existing charcoal furnaces* was in the late 1770s or 1780s. He concludes "So old-style bellows at Hopewell in 1771 wouldn't be surprising, though my guess is that an up-to-date ironmaster would be installing cylinders in a new furnace." Mark Bird's father was already in the iron industry, so Bird was certainly informed; whether he was "up-to-date" is less clear. King is less certain about the period introduction of cylinders across the ocean, pointing out that "Blowing cylinders were only introduced in England in the late 1750s." and "Unless someone traveled to America who knew the new designs, it is likely that Hopewell started with the traditional bellows."

The question of single vs. double acting cylinders must also be considered. This improvement is almost certainly a matter of the ability to make the device as opposed to imagination, so the double-acting blowing cylinder is likely to be related temporally to the double-acting steam engine. Watt made his first such engine in 1783, but was thinking about it by 1774 ([Dickinson 1927], p. 139). A hint at the lack of prior double-acting cylinders comes on p. 141, where he discusses the new problem of connecting the piston to the beam when the connection can now be under compression in addition to tension. One obvious means is "a guide for the end of the piston rod; ..." i.e., a crosshead. But, "the production of long straight surfaces in metal by hand, however, was a long and expensive process; ..." This may help explain the use of single-acting cylinders where the piston rod exits the open end of the cylinder, although not cases where the rod penetrates the closed end.

Dickinson on p. 110 states that Watt was designing a blowing engine for John Wilkinson in 1775, the Broseley Blowing-Engine. (Schubert on p. 333 states that it was installed in 1776.) Since this was single-acting, it means that steam blowing did not wait for a double-acting engine. The diagram for this engine on the plate opposite p. 112 shows a single-acting blowing cylinder. In this case the piston rod is through the closed end of the blowing cylinder, although the traditional semi-circular head and chain are used on the beam to keep the rod straight. Additional comments are on p. 245, along with the claim that "No earlier instance is known of the direct application of the steam engine to a blowing-cylinder than this of Wilkinson ..." On p. 246 he states that in 1793 a blowing engine was erected at the Neath Abbey Ironworks that was double-acting for both steam and air. There is no comment as to whether this was or was not considered the first double-acting blowing cylinder known for blast furnaces. He also states that in August 1802 a blowing engine started that used a crosshead and no beam.

Further information on the state of the art in Britain around 1800 is found in Rees's Cyclopaedia, easily indexed through it's Wikipedia page. The `archive.org` versions are used here. The relevant articles are in Volume 4 [Rees 1819] and the plates are in Plates Volume 2 [Rees 1820]. However, the notation on the most relevant plate indicates publication on 2 January 1802, so the material reasonably represents the state ca. 1800. The article on blast furnaces starts on p. 548 of the online reader and downloaded PDF file. P. 549 indicates that the commonly known furnace is a coke one with a steam blowing engine. This is described on p. 552 and plates p. 76, which is "Chemistry Plates II." This has a single single-acting blowing cylinder that blows up with a closed top, into a regulating cylinder (receiver at Hopewell) with a weighted "fly" piston.

Rees on p. 563 relates that the furnace started ca. 1760 at Carron did poorly on coke when using large bellows and water wheel. At an unspecified time the bellows were abandoned "and in their place large iron cylinders were introduced blowing both up and down." This indicates that a British ironmaster would still install leather bellows ca. 1760. Note that whether the cylinders are double-acting is not clear; as with the "double acting box bellows," different cylinders could act on the up vs. down strokes.

The Rees article on "Blowing Machine" starts on p. 632. Pp. 632–633 states that these used first wood cylinders, then bored cast iron cylinders. "This took place nearly 40 years ago, and continued with a few temporary deviations until the introduction of Bolton and Watt's highly improved engine." Since the articles were written in the early 1800s, this is consistent with Dickinson's statement about the first direct steam/cylinder blast being in 1775. Rees then describes the configuration shown in the above plate, and continues "The chief objections to this mode of blowing, even when in universal use, were founded upon the great inequality of the blast, ..." His article in general implies that he believed that the single-acting cast iron steam-powered blowing engine, with a regulator, was the standard blast in Britain in the early 1800s. However, on p. 635 Rees states "since the period of the introduction of Mr. Watt's engine, the air-pump, or blowing cylinder, has been constructed so as to discharge a cylinder full of air on every ascent and descent of the piston." and then "Formerly, in the common atmospheric engine, the movement of the piston from top to bottom, and back again, produced only one cylinder full of air from the air-pump, ..." This clearly describes a transition from single to double acting blowing cylinders. So it appears that ca. 1800 is a transition period, where double-acting blowing cylinders were in use, but apparently not common enough for the encyclopedists to show them as the canonical form.

15.2. American Practice

As was related in Section 3, Thomas Russell had a drawing of a cylinder blast in 1757. However, reading Robbins chapter 2 in general leaves uncertain whether Russell was associated with the Principio Company at that time, although per p. 67 he was a son of one of the founders of the company. He was apparently in England in 1757 (p. 169) and was sent to America in 1764 to become Principio's general manager (p. 67). This suggests the possibility that Americans ca. 1760 could have been aware of such a blast through the Principio route, although the apparent lack of action leaves doubt.

Later, Russell wrote a letter to all Principio partners dated 22 June 1772, which indicated that the Lancashire furnace had blast problems: "the blast is generally but indifferent owing to the stream of water being small and the Bellows proportionate thereto, which makes me inclinable to have wooden bellows as the Blast is allowed by all who have used both to be much superior in force to the leather kind." This is a clear statement that the furnace was currently using a leather blast. It is also a claim that wood blasts were well known, and that that their advantages were generally known. In December a cylinder blast is apparently in design or being installed, and on p. 171 Robbins quotes from a letter dated 26 May 1773 to the company clerk, "Lancashire furnace is blowing, the Celinders seem to answer extremely well." Robbins on p. 173 states that "Whatever the design of the 'celinder' at Lancashire Furnace, they were among the very first such devices installed in any colonial ironworks." However, no reference or justification is given for "among the very first." In summary, someone possibly associated with a Maryland company was thinking about a cylinder blast in 1757, one was installed in Maryland in 1773, and is said to have been among the first such in the colonies.

Peter Kalm was a Swede who traveled in the region in 1748–1749. He was a botanist, but also observed other things. An English translation of his account was published in two volumes [Kalm 1773]. In Volume 1 p. 131 he passes a forge two miles behind Chester, PA, which, based on Graham, is likely Crum Creek Forge. He states that "The bellows were made of leather, and both they and the hammers, and even the hearth, but small in proportion to ours." In Volume 2 p. 249 he describes a forge of six fires three miles west of Trois Rivieres, Quebec. He states that "The bellows were made of wood, and every thing else, as it is in Swedish forges." While not close to Hopewell, this shows a wooden blast in the northeast ca. 1750, predating Hopewell Furnace by two decades. He also implies that leather blasts were obsolete in Swedish forges at the time.

Several authors make general claims as to when blasts changed. Eggert on p. 8 states that "Beginning at the start of the nineteenth century, closed blowing cylinders or blowing tubs, still powered by water wheels, began to replace them [leather bellows]." However, no evidence is cited. Others have been related above, and are summarized here. Hermelin claimed that most American forges used wood cylinder blasts ca. 1783, apparently based only on his observations or hearsay. He claimed the casting of blowing cylinders at Cornwall the year of his visit. Gordon states that most ironmasters used wood blowing tubs by the 1780s, but he references Hermelin, whose statement was only for forges. Pearse states that wooden blowing tubs were introduced not long before the Revolution, but does not make clear whether this was for forges only. Swank states that "about the time of the Revolution" wooden cylinders were also used, again, without making clear whether these were also for furnaces.

An American version of Rees was also published and not as a mere reprint. However, the blast furnace and blowing machine articles appear to be about the same as the originals.

15.3. Regional Practice

The final analysis of practice was to research specific furnaces in the immediate area, to find blast details that were not included in the more general descriptions. This started with a review of general books and articles to check for descriptions of specific furnaces. Then, a list of furnaces built by 1830 was made for Berks, Chester, Lancaster and Lebanon counties. The list was started using the website `http://paironworks.rootsweb.ancestry.com/`, and this was augmented from other sources. Searches were done on each furnace, as described below.

Daniel Graham has written several summaries and library finding aids for south-eastern Pennsylvania forges and furnaces. The one for Chester County [Graham 2006] contains almost no blast descriptions. He recites part of Schoepf's description of the blast at Coventry Forge (p. 11) and quotes from Kalm concerning the Crum Creek Forge. His description of Coalbrook Dale Furnace and Pine Forge [Graham 2010] has nothing about blasts, and in general concerns ownership rather than technical details. His description of Mongomery County forges [Graham 2008] is similar, with no information about blasts. His finding aid for the Historical Society of Pennsylvania collection [Graham 2005] contains summaries of the various furnaces and forges included. The only mention of a blast is for Pine Grove Furnace: on p. 56, "and had, in 1870, the last extant specimens of the old single-acting wooden blowing tubs."

Alfred Gemmell wrote a summary of forges and furnaces in the Perkiomen Valley [Gemmell 1949]. This contains virtually nothing about furnace blasts with the exception of Dale Furnace in Berks County, active 1791–1822. On p. 59 he states "to operate the two big bellows producing the blast." However, he states that not even basic stack dimensions are available, calling into question whether "big bellows" is just generic instead of implying leather.

As related earlier, Boyer has described cylinder blasts at two New Jersey forges. The first was an apparent multi-cylinder single-acting iron cylinder blast in Boonton in 1794. The second was at Dover Forge ca. 1810. Boyer also has some specifications for Oxford Furnace (NJ) starting on p. 148. This includes a comment about larger "bellows" in the early 1800s, and a tub blast in 1832.

Bining does not include sections on specific furnaces, although certain attributes of various furnaces are mentioned. No mention is made of the blast details for specific furnaces; there is only the general section on blasts on pp. 70–72, as outlined previously.

Swank includes regional sections with forge and furnace descriptions, and was reviewed for the northeastern states down to Delaware and Maryland. There is almost no information on blasts. On p. 187 he states that "Warwick and Cornwall furnaces, two of the best furnaces of the last century, retained their long leather bellows until the present century." (This means into the 1800s.) On pp. 188–189 he quotes from a sheriff's sale notice for 1769 for Martic Forge in Lancaster County, PA: "with four fires, two hammers, and very good wooden bellows ..."

Pearse also says little about specific furnace blasts, and adds nothing not already stated in other sources.

Lesley [Lesley 1866] has an extensive list of furnaces active in the early 1860s. However, the standard description includes size, ore and capacity, but no mention is made of blasts.

The Colonial Dames of America published a book written by a research committee on Pennsylvania forges and furnaces in the colonial period [Committee on Historical Research 1914]. It says almost nothing about blast machinery. On p. 46 it describes Durham Furnace (Bucks County) as having "large leather bellows," and is apparently describing this ca. 1727. On p. 97 Kalm is quoted again as describing Crum Creek Forge in 1749, "The bellows were made of leather ..." Finally, p. 137 repeats the Martic Furnace 1769 sheriff's sale.

As a final check, a survey of area furnaces was done, looking for furnaces in Berks, Chester, Lancaster and Lebanon counties, that were built by 1830. Individual searches were done for each for literature specific to each that described the blast. Three searches were done. The most general was a simple Google search for "X furnace." The second was a Google Scholar search for the exact phrase "X furnace." Finally two HathiTrust searches were done for the exact phrase "X furnace." The first was only on the title, which most of the time got no hits. The second was for full text and all fields, which frequently got tens to hundreds of hits; only the first page of 25 was inspected. Very little was found, and except for the Miller work on Cornwall Furnace, all hits that included information on the blasts were in the multi-furnace books and articles described above.

Cornwall Furnace was a nearby and mostly contemporary furnace, described by Miller in chapter 5 of [Miller 1950]. It was first blown in during 1742 (p. 87) and built with water-powered double leather bellows 20 ft long by 6 ft wide (p. 104). In 1847 a steam blowing engine was installed (p. 108) and the wheel relegated to backup status. In 1856–1857 a new blast was installed consisting of two blowing tubs, described in pp. 109–110. The description seems to indicate single-acting tubs with one end open, but this is not certain. The description implies that the piston was also built like a tub, out of staves. There is no mention of a crosshead. The two tubs blew into a receiver, which had a weighted piston for pressure equalization (an air regulator in British parlance). P. 113 has an excerpt from an interesting letter from John Miller, a "bellows-maker," dated 15 December 1786. This references proposed furnace bellows repairs to be done and the cost. Apparently, someone in the area was making at least a partial living off of furnace bellows work, which in this case probably meant leather.

In addition to these, Oxford Furnace in Oxford, NJ is described in [Warne 1991], with slight information on the blast. The blast was apparently replaced in 1832 with a second-hand claim that the new blast used tubs, and no leather was in the list of material purchased. Warne does not know what the previous blast was, but quotes a "tradition" that the original blast was a trompe (water blast).

The detailed research into local furnaces produced no new information. We still see leather furnace bellows in the earlier 1800s, wood forge bellows prior to Hopewell's construction, and furnace blast conversions into the 1800s.

15.4. Summary

This section provides little information that informs the original Hopewell blast design. We see British blowing cylinders in forges starting in the 1730s and in furnaces starting in the 1750s. Steam blowing engines start in 1775, and double-acting blowing cylinders by 1793. We see cylinders in America in a forge ca. 1750 and at a furnace at Principio in 1773. Then there are general statements, without support, about cylinders appearing around the time of the revolutionary war, and that iron cylinders were cast about the same time. This puts the date of Hopewell's construction in the transition period. It would seem likely that Mark Bird, coming from an iron making family, would have heard about cylinder blasts, but this does not mean that the new technology looked like a good bet at the time he built Hopewell. So, the above data is not telling us whether the original Hopewell blast was leather or cylinder, but says that either is plausible.

Of greater interest is that none of the descriptions of the early cylinder blasts are for double acting cylinders. All are single-acting, although diagrams show both open top and open bottom cylinders. The earliest date seen for a British double-acting blowing cylinder is 1793. If Hopewell was built with a cylinder blast, it was almost certainly a single-acting design rather than the current one. Further, if it was originally built with a leather blast but converted to cylinders prior to 1800, it was also very likely a single-acting design.

16. General Discussion and Conclusions

As is clear from reading the official reports, there is dispute and uncertainty as to the blast design over the life of the furnace. A particularly telling point is the lack of evidence for an early blast from the archeology. While the excavations discovered the north-south wheel pit and various walls, they did not turn up any evidence for any particular blast. The only direct statement about this is the speculation that scattered foundation fragments might have supported a leather bellows blast, without any statement that the speculation is directly supported.

There is a general lack of evidence that is capable of establishing blast designs in isolation. The only blast whose design is absolutely certain is the current one, being the same as at the end of the historical period. In addition, it is concluded in isolation that *by* 1822 the blast was a cylinder blast utilizing pistons with rings. However, when various data and analysis are combined, additional conclusions can be made, although with varying levels of confidence, and all short of certainty. This section presents the conclusions that might be drawn.

16.1. Broad Summary of the Report

A broad summary of the major sections of this report is:

- The word “bellows” cannot be assigned any meaning besides “the blast machinery.”
- The various historian claims are consistent with the general history of blast evolution.
- The reports of Hopewell historians do not justify their conjectures and conclusions, so conclusions must be re-derived from evidence.
- The extracted data is not for the blacksmith bellows.
- The 1883 blast design is known, being the current blast.
- What is known about the west headrace conversion is not helpful.
- The existence of an earlier north-south water wheel is established, but it was not 30 ft in diameter. It may have approached 30 ft, but the margin is not clear. It was almost certainly at least 20 ft, a range which is not very helpful. The wheel conversion may contain useful information.
- The “patent elastic piston springs” mentioned in 1822 are for pushing a piston ring out from the piston, and hence fix the general blast design as being a cylinder type at that time.
- The “bellows dressing” data is significant and indicates changes to the blast.
- The meaning of various terminology cannot be sufficiently fixed to inform blast design.
- The records for millwrighting provide some insight into wheel and general work.
- The general blast practice of the prehistoric period places Hopewell’s construction in the transition period from leather accordion to single-acting cylinder blasts.
- The production data discounts the idea of an inferior blast prior to 1816, and does not conflict with my discounting of prior blast conjectures.

16.2. Specific Conclusions

The following are specific conclusions that are drawn based on combinations of data and analysis. As stated before, most of these cannot be considered certain.

Note that the early, middle and late periods refer to those defined by the dressing data, *approximately* 1800–1840, 1840–1850 and 1850–1883.

The entire late period likely used the current blast, except possibly for a smaller receiver prior to 1881. This period by definition ended with the current blast. Throughout, there is no record of buying bellows leather or dressing the bellows, so this data is stable for the entire period. There is no record of significant millwork, there is what seems only maintenance or repairs on the blast. The book entries uniformly use terminology consistent with the current blast, such as "tubs," "piston rod" and "crank."

A blast improvement between the middle and late periods is likely. The clear support is the change in dressing statistics. In addition, purchases were consistent with blast renovation, including bellows leather, a "tin blast pipe" and "copper pipe". Minor millwork is recorded for the transition period, and there are entries that appear to be for parts replacement, repairs and/or maintenance. This implies that the blast was already substantially in its final form. The claim is that improvement in detail reduced the need for blast maintenance.

The entire middle period likely used the same blast. Like the late period, there were no leather purchases except for an unspecified amount at the end, the dressing statistics are constant, and there was apparently nothing of consequence bought for the blast. There was no millwork and the entries present seem for repairs and maintenance.

The blast was almost certainly a cylinder blast prior to 1822. The spring analysis leads to this conclusion, with the additional evidence of the Chestnut Grove Furnace description. Further, the Chestnut Grove statement suggests a single-acting blast. No claim is made that the blast was, in detail, the same as the current one. Although at least one prior Hopewell historian apparently believed the conversion was done *in* 1822, this is not supported by the data. There is no mention of millwork or bellows leather purchase in 1822, and the statistics of bellows dressing did not change over 1822. A conversion then is unlikely.

A significant blast improvement between the early and middle periods is likely, and most likely in 1838. There is a clear and significant change in dressing statistics, plus significant millwork in 1838. This cost was less than half the estimated cost of a wood cylinder blast, so a completely new blast is not supported, but significant work is. The leather purchases at the end of the early period imply a re-leathering as part of the improvements at the transition. The implied improvements are likely the source of reduced dressing needs in the next period.

A consistent blast during the early period is likely. By definition, the dressing statistics are consistent over the period. There is no mention of sufficient work or expense to support a new blast anytime in the period. Bellows or piston springs are mentioned in 1822, 1830 and 1831. Given the likelihood that the bellows springs are piston springs, this confirms a cylinder-type blast for the second part of early period. Finally, the individual evidence against both a new blast and a wheel conversion reinforce each other. Since a suitable blast conversion is the only plausible reason for a wheel conversion, they should occur together or not at all. So evidence for a blast conversion would require evidence for a wheel conversion also, and since the evidence is against both individually, a blast conversion is unlikely.

The claims of a blast conversion ca. 1805 are not supported and unlikely. There is no work mentioned in this period besides dressing and wheel repairs. This is not remotely close to what would be required for a new blast and wheel conversion.

The R&A claims of a blast conversion ca. 1818 are not supported and unlikely. The only blast-related work in 1818 is dressing. It would also be very odd to have done all the work in 1816 on an old blast, only to replace it with a new blast two years later, without any mention of it.

The claimed $8,000 in repair costs in 1816 is overstated by at least a factor of 10. As described earlier, the figure seems absurd on it's face, and study of the books comes up with only $500–$600 in costs.

The 1816 repairs did not include a new-design blast. The records contain no direct mention of any work associated with a new blast. There is significant unspecified millwright work that took place in the summer, most likely over a period of $2\frac{1}{2}$ months. As noted, about a third of this is likely allocated to a new wheel. The remainder is only about a third of what Overman states to be the cost of a new blast of the probable type. The amount is plausible for refurbishment of a blast that had been neglected for eight years. Less solid evidence is that the dressing statistics seem the same for the periods before and after 1816. This is less solid due to only three datapoints prior to 1816, although the unspecified bellows work in 1804 is plausibly similar, and would provide the same density of datapoints before 1816 as after.

The 1816 repairs likely included a new water wheel but not a conversion. Book entries for wheel repair are frequent in the first decade of the 1800s and then stop abruptly in 1816. Later data (including for modern times) indicates a wheel life of approximately one and a half decades, with a clear new wheel in 1830–1831. This indicates a new wheel in 1816. The total millwright time for the summer is about three times that necessary for a new wheel, and the remainder is plausible for blast repairs, so a wheel is consistent with time records.

The records are not consistent with a wheel conversion at this time. There is a record of masonry work for a wall, but only for a fraction of what would have been required for the new wheel pit. There is no mention of any work specifically for such a task.

A wheel conversion, or even replacement, in the first decade of the 1800s is very unlikely. The wheel data shows wheel repairs in almost every year of operation for the first decade, but nothing for millwright work or a new wheel. The abrupt end of wheel repair costs after 1816 indicates that a proper wheel needs little repair work. This is convincing evidence that they were nursing a troublesome wheel rather than replacing it. Historian claims of a wheel conversion in this decade seem to be based on possible dates for a new west headrace and the claimed association of that with a wheel conversion. As noted, I dispute this association.

A wheel conversion in 1838 is unlikely. First, there is no mention of a new wheel or a new wheel pit, both of which would have been significant expenses. Then, the only plausible explanation for a wheel conversion is held to be a change in blast design from a ground-mounted one to an over-wheel one. While a significant modification to the blast is very likely in 1838, the claimed cost is only half of what Overman gives as the minimum cost for a new blast of the type likely being updated. This indicates a lack of reason for a wheel conversion. Finally, the fact that the 1830–1831 wheel was relatively new argues against this.

The Abel claim that the tubs were installed ca. 1851 is disputed. The claim appears to be that of the blast conversion, tubs replacing leather bellows. This is contradicted by the conclusion of a conversion by 1822. While not explained, the claim seems likely based on the "tub" language appearing at this time in the books. As noted, no significance can be attached to this. This does not dispute that modifications were made to the blast around this time.

The claims of a Coventry blast are disputed. No evidence for this blast was seen, only historians conjecturing that it existed. A particular problem with the Coventry conjecture is that, taken as written and without further information, it is a single cylinder and almost certainly single-acting, possible for a forge but useless for a furnace. The Schoepf description is sketchy in the extreme, and Apple offers no elaboration. It is of course possible that there were multiple cask pairs instead of only one. However, the uniform repetition of the single-unit Schoepf description suggests that nobody else ever saw one of these installations or any evidence therefore. If there was any evidence of a multi-unit blast at Hopewell, it is unlikely that the historians would have repeated someone else's description of an apparently useless single-unit version from somewhere else in favor of actual local evidence. Further, the repeated use of the "four posts" description leaves one looking for archeological evidence of the four post holes, but nothing of the sort is mentioned.

There is no evidence for a leather bellows blast, and if one existed, it was likely replaced prior to 1800. Claims that Hopewell ever used a wood and leather accordion blast are stated as assumptions, with no evidence provided. The assumptions appear to be based only on historical precedent, which is certainly plausible. The furnace construction date is in the transition period from leather bellows to cylinder blasts, so cylinders should have been both known and new to the builders. As noted above, some early single-acting cylinder blasts used the same general layout as a leather bellows blast, so it is possible that no remaining archeological evidence could indicate the difference. However, archeology did not turn up any evidence for any specific ground-mounted blast, just bits that could be construed as foundation. Finally, the previous conclusions are that the blast was of a consistent design for the early period, and that it was clearly a cylinder blast by 1822. This pushes any conversion from a leather bellows blast into the prehistoric period.

The bellows preparation process on p. 31 of Raistrick also adds doubt to the idea of a leather bellows blast in the historical period. This is taken over a period from 1690–1750, so it is contemporary with Hopewell. This implies that the bellows were overhauled before each blast, including buying hides (bellows leather) and doing significant work. If this represents the amount of work required during the early Hopewell era, it is not being found in the account books. Dressing is sporadic, and purchase of bellows leather is more so.

There is circumstantial evidence for an original ground-mounted blast different from the blast assumed to exist ca. 1800. The wheel conversion is deemed unlikely after 1800, but seems to need a blast conversion to justify it. While there is no direct evidence that a presumed single-acting cylinder blast present ca. 1800 was *not* original equipment, blast consistency eliminates the only clear justification for the wheel conversion. However, a ground-mounted blast may have been converted to an over-wheel single-acting cylinder blast in the prehistoric period, driving (possibly necessitating) the wheel conversion. However, a ground-mounted blast could have been either leather or cylinder, and these two options could have had the same layout. Note that an over-wheel cylinder blast would have been directly driven and hence possibly considered an improvement over a ground-mounted cylinder blast, which more likely would have been partially counterweight-driven. So the conversion of the latter to the former is plausible.

There is circumstantial evidence for the blast and wheel conversion in the mid-1780s. The seemingly continuous wheel repairs recorded for 1800–1808 indicate an old wheel, although incompetent construction could also be a factor. Assuming proper construction, later evidence is indicating a lifetime on the order of a decade and a half, with few recorded repairs. The high repair count plausibly indicates a wheel at the end of its life around 1800. This would indicate a construction date in the mid-1780s. This time frame is also consistent with the nominal life of the original wheel.

The original north-south wheel was less than 30 ft in diameter, but otherwise uncertain. Despite the general acceptance of the 30 ft figure, the only stated source is Harker Long, and no archeological evidence is quoted. There is neither evidence nor claims that the old west headrace was higher than the east headrace, and claims that it was lower. The elevation of the east head race pipe entrance grate makes it very unlikely that the wheel could have been 30 ft, but how much less it was is not clear. The pipe elevation profile is most consistent with the existing wheel, so it is likely masking the elevation of the original race, leaving no evidence of the original wheel diameter.

16.3. The Most-Likely Sequence

Given that: there was a cylinder blast in 1822; dressing statistics did not change until ca. 1838; descriptions of early cylinder blasts are all single-acting; "Dotterer tubs" were said to be single-acting; and that a significant amount of millwrighting was done in 1838; *it is likely that an existing single-acting cylinder blast was converted to something close to the current double acting blast during the 1838 work.* Given that the diagrams of earlier blasts frequently do not include crossheads, *it is plausible, but not proven, that crossheads were added in 1838.* Crossheads are certainly desired for double acting cylinders, and their addition is a good explanation for the significant decrease in dressing needs. Recall that Overman's "wooden blast machine most usually made" had two single-acting wood cylinders and no crossheads. This is reflecting mid-century practice, so an 1838 conversion is reasonable. Per the power analysis, conversion to a double-acting blast could also have reduced cylinder diameter, stroke and wheel speed, all of which would have likely reduced wear and hence dressing.

Since there was no significant early 1800s millwork except in 1816, and the dressing statistics are similar before and after 1816, *it is likely that the blast was a single-acting cylinder blast starting before 1800.* This was also more likely than not an over-wheel blast, since that would have justified a prehistoric wheel conversion. Also note that historian claims of an inept early blast are not supported by the data: little blast repair is recorded, although there are frequent wheel repair entries, and high production was recorded.

Finally, *the prehistoric blast was most likely ground-mounted, either a single-acting cylinder blast or a leather bellows.* The preference for a ground-mounted blast is that it's conversion to an over-wheel blast provides the only clear justification for the wheel conversion. No preference can be given to the cylinder vs. leather blast. The cylinder blast would be consistent with available technology ca. 1771, but using the traditional and hence well-known leather blast would be consistent with the relative newness of the cylinder blast for furnaces. Both could have been consistent with the same general layout and use of the north furnace room. An original over-wheel cylinder blast is still not ruled out, but this creates the problem of explaining the wheel conversion. Note that "ground-mounted" means not over-wheel and not necessarily with cylinders directly on the ground. They could still have been elevated with a crankshaft underneath and have both cylinders on the same side of the wheel, fitting in the north furnace room.

Note that the above does not address *why* the blast was changed, for which there is no clear evidence. The production data does not support the idea that upgrades were essential to increased production, as blasts of high and low production are seen throughout the furnace operating life. This same argument applies to upgrades to increase blast efficiency. The most likely explanation is reliability and maintenance. The dressing statistics clearly support this for the historical period. If leather bellows were the original equipment, the same reason would justify converting to a wood blast. If a ground-mounted cylinder blast were original, the over-wheel blast using direct drive by piston rods would probably have been an improvement over a crankshaft, lever arm and counterweight arrangement.

In summary, the most likely series of blasts, with varying levels of evidence, is considered to be: an initial ground-mounted leather or cylinder blast; a conversion to an over-wheel single-acting cylinder blast prior to 1800 (with light circumstantial evidence for the mid 1780s); a conversion to an over-wheel double-acting cylinder blast ca. 1838; minor improvements made ca. 1850; and a larger receiver installed ca. 1881, resulting in the current blast still in use today.

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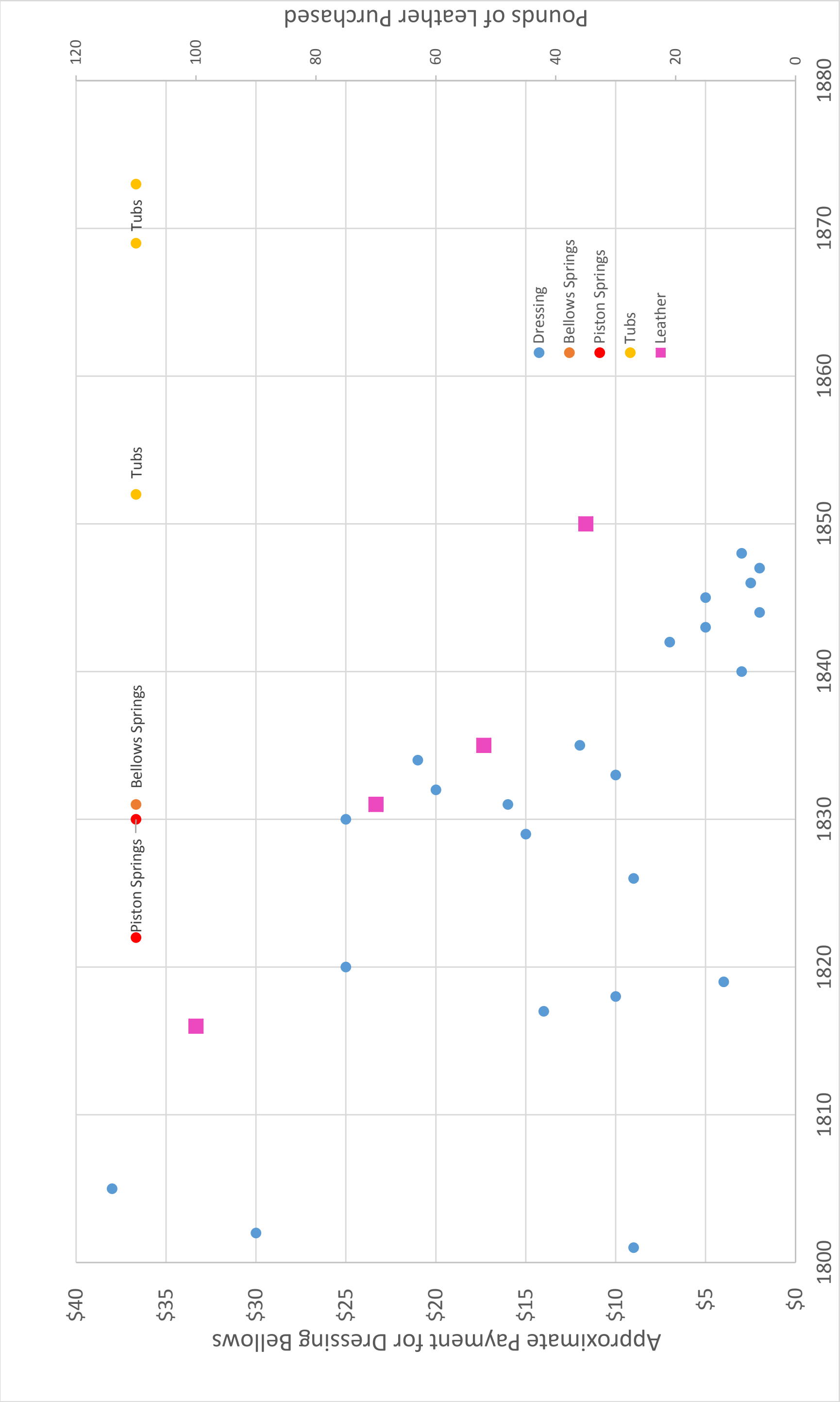


Table 5. Approximate Spending on Bellows Dressing, plus flags for certain activities.

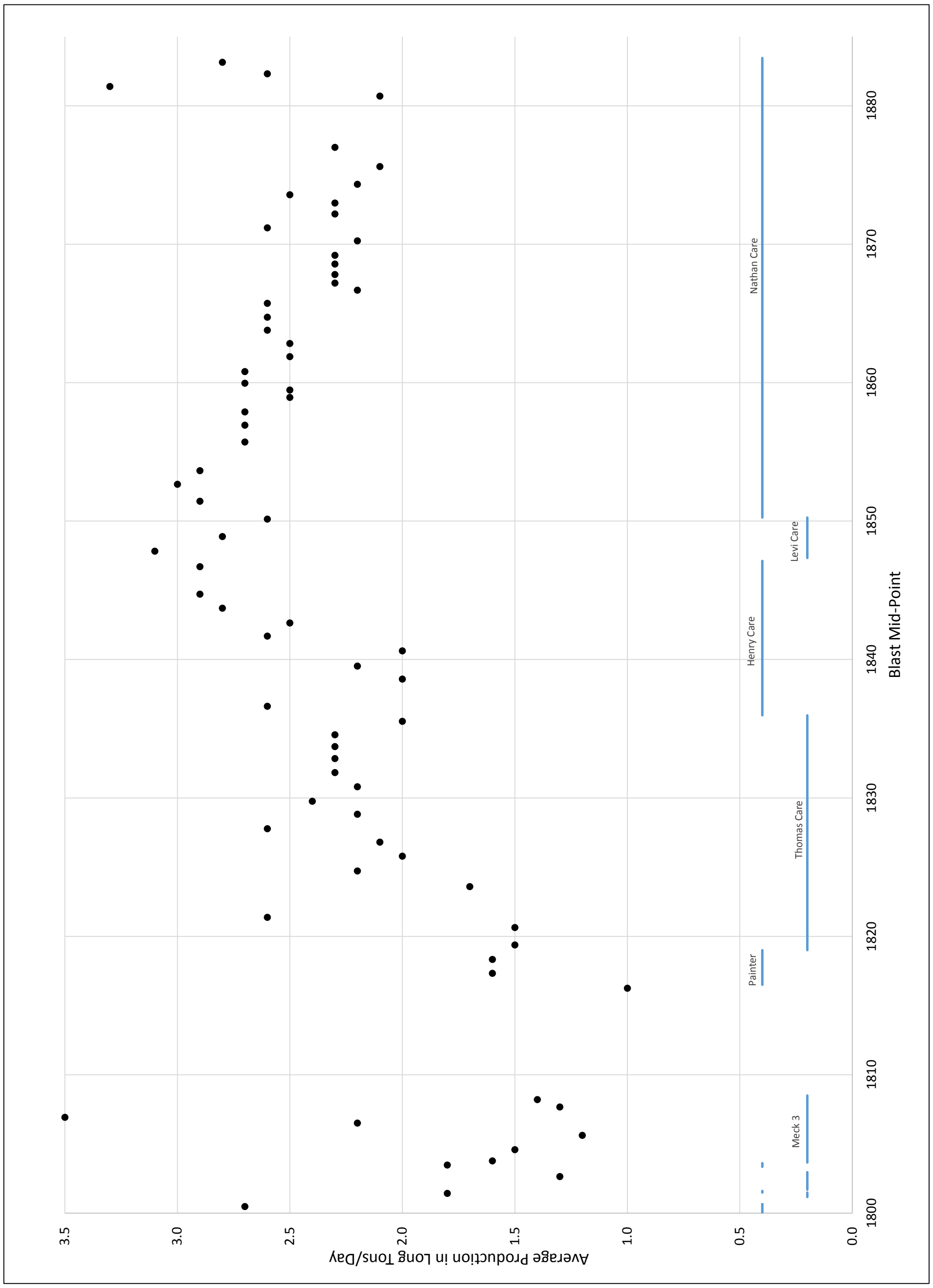


Table 6. Production in Long Tons/Day for each Blast Period.

19. Appendix: Table of Significant Conclusions

This is a tabular summary of the changes in conclusions. Note that most "conclusions" are actually "likely" or similarly qualified, as no new evidence was found that would prove any blast design with certainty.

Original Conclusion	New Conclusion
Original wheel was a 30 ft diameter overshot.	Range was from 20 ft to under 30 ft, but not 30 ft.
The west headrace was relocated 1790–1810, and related to the wheel conversion.	The race and wheel conversions were not necessarily related. The race relocation dates are not important, and the originals as good as any.
The wheel was converted from north-south to east-west 1790–1810.	The wheel was converted prior to 1800, with light circumstantial evidence for the mid 1780s.
The initial blast was or was assumed to be a double leather bellows.	The initial blast was likely ground-mounted, and a double leather bellows or at least two single-acting cylinders.
The initial blast was converted 1790s–1822, most likely to a Coventry type.	Conversion was prior to 1800 and with the wheel. There was never a Coventry blast. This was likely an over-wheel single-acting cylinder blast.
$8,000 was spent on repairs in 1816.	No more than $800 was spent, and possibly only about $500.
The blast was converted to double acting in 1816, 1818, 1822 or ca. 1851.	The conversion was likely done in 1838, to a design close to the current blast.
Various unspecified blast improvements were done at various times.	There is evidence only for an improvement ca. 1850.
A new wheel was built in 1879 and a larger receiver was installed in 1881.	The only evidence is Harker Long, but these are accepted.

20. Appendix: Excerpt on The Blast Machinery Restoration

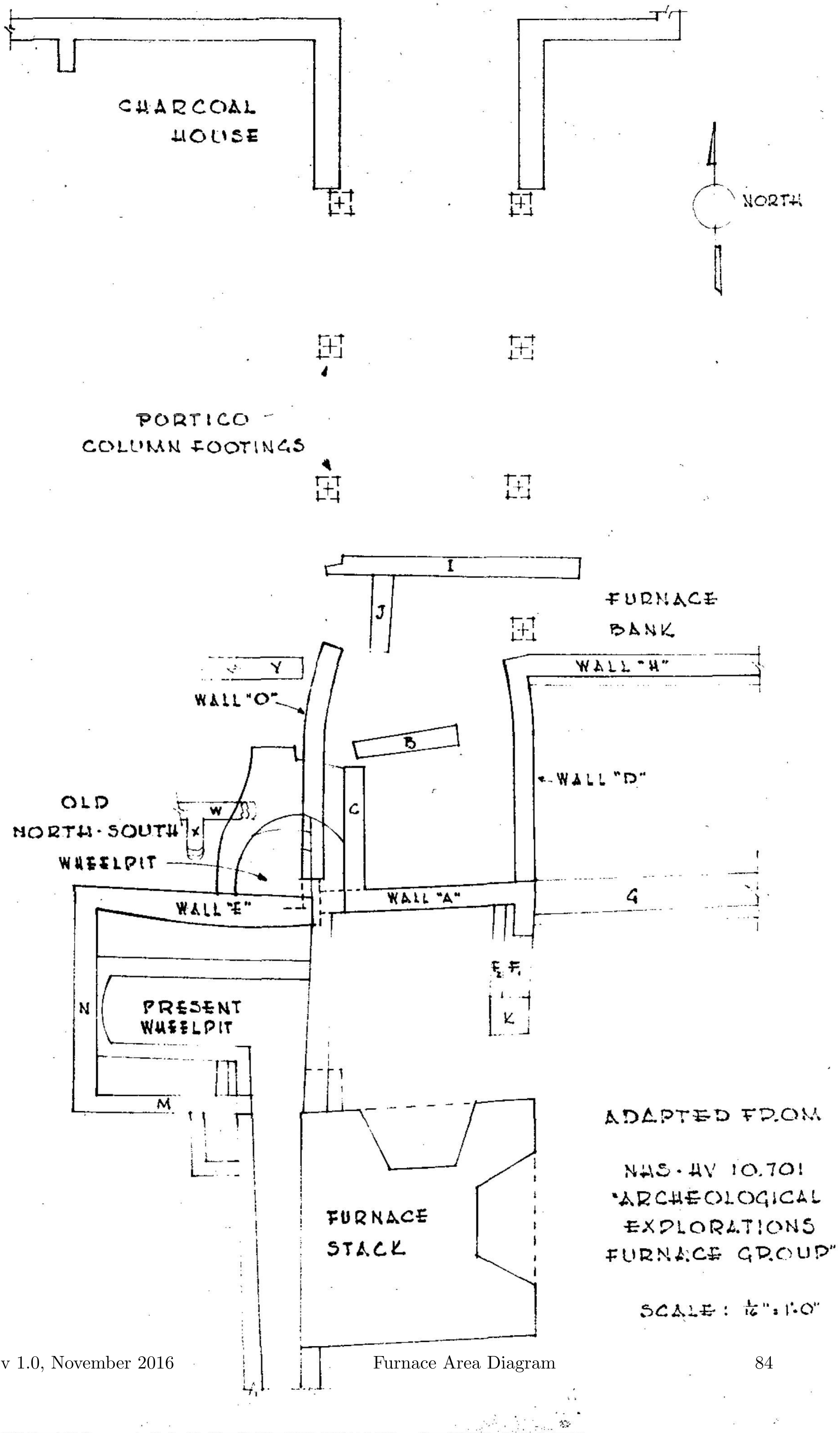
This is a verbatim copy, except for formatting, of the parts of pages 10 and 11 of the Cass/Hugins report [Cass 1952] that deal with the restoration of the blowing tubs and related equipment.

While the wheel parts were being air dried in the barn, work was begun on the blast machinery. Some of the historic machinery was still usable after repairs had been made, in spite of having been left out in the weather for nearly twenty years. The old receiving box was in fair condition, although new wooden yokes and a new top had to be made, and the interior had to be releathered and sealed with canvas tape. One of the old blowing tubs was also in fair condition, although its piston had to be rebuilt. Repairs to this tub included new valves and leathers, resanding and cleaning of the interior, and tightening and readjusting of the iron bands which circled it and held it together.

One tub and its piston had to be completely reconstructed. This was a job, not only for a skilled cooper, but for a "thinking mechanic," since the drawings of the old machinery were seldom in sufficient detail to answer all questions which arose. While the construction of the water wheel and framework timbers was basically simple carpentry, restoration of the blast machinery called for precision work, all parts fitting together with small tolerances.

The blowing tub was made of dry, well-seasoned white pine boards (2"x4.5"). The edges of each board were beveled to make a fit, and glued together to form a cylinder about six feet in diameter. The inside of the tub was than sanded to make it smooth and perfectly cylindrical, and the top and bottom of the tub, both laminated, were constructed. The closely fitting pistons for the two tubs were also made of white pine, laminated in four layers. The boards in each lamina were laid at a different angle, one being made up of triangular segments. All joints were sealed with canvas strips, and the sections were then bolted together. The leather piston wall was then nailed around the perimeter, held tightly against the cylinder by ash piston rings, one on each side of the double-action piston. To compensate for wear and prevent air leakage, each ring could be adjusted by eight piston ring braces and blocking and two piston springs. The old springs had crystallized, so new ones had to be made at a machine shop. The valves, two for each blowing tub, were made of pine lined with leather, operated by wooden valve floats.

After assembly the cylinder walls were coated with plumbago or graphite, mixed with soapstone powder and glue; this decreases friction at the same the that it maintains a close fit of the piston. With the tubs and receiver in place, tinsmiths from Reading connected the tuyere or blast pipe system, which after weathering was painted with black graphite paint to simulate the original. All wooden parts not treated with creosote were treated for durability with pentachlorophenol, and the blast machinery was completed.


CHARCOAL HOUSE
NORTH
PORTICO COLUMN FOOTINGS
I
J
FURNACE BANK
Y
WALL "O"
WALL "H"
B
WALL "D"
OLD NORTH-SOUTH WHEELPIT
W
X
C
WALL "A"
G
WALL "E"
F2 F1
K
N
PRESENT WHEELPIT
M
FURNACE STACK
ADAPTED FROM
NHS-HV 10.701 "ARCHEOLOGICAL EXPLORATIONS FURNACE GROUP"
SCALE: 1/16" = 1'-0"