My name is Bo Jensen. I work for Kroppedal Museum. In this talk, I want to briefly introduce a site we excavated a few years ago, called Alpegård. This is yet another of these sites with more than a thousand cooking stone features (CSFs). My focus today is less of this particular site than on our methodology: we tried a data collection strategy that was probably better than anything *we* have done previously, but was not therefore perfect. I offer it here, not as the perfect solution but rather as an inspiration and as a case study where you may also learn from our mistakes.

So, first, the site: over some 20 years, we have excavated a number of areas on this plateau (MAP, MAP). Importantly, until the most recent excavations, we had discovered only around 100 CFSs. Then, on this small area of the north-western corner, on a north facing slope, we found a thousand. This is clearly a very localized phenomenon. As I shall argue momentarily, a lot of this activity dates to the Late Bronze Age and Pre-Roman Iron Age. On the most recent excavation, we also found a large number of houses, but nearly all of these are later, mostly Migration Period. Only some five date to the LBA/PRIA. We do have a fair few houses from the LBA/PRIA on the older excavations, notably on the south-eastern part of the plateau, so there seems to be a spatial separation between CSFs and contemporary houses.

As you can see, the CSFs form multiple concentrations. Notably, there is a big belt of rows just below the top of the hill, and a number of smaller concentrations further down the slopes.

We have a fairly solid C14-chronology, and I am confident that the CFS phenomenon covers the period 1200 BC to 400 AD, calibrated. That is to say that we have pits as early as any known, anywhere, and as late as any known in Denmark. This one site seems to capture the whole chronology of the CSF phenomenon, in Denmark.

We have excavated just over 1000 pits. There may be anything up to perhaps twice as many in total, including the area west of our excavation. However, even if we have 2000 preserved pits over 1500 to 1800 years, that averages as just over one pit per year. We are looking at long chronology of very low intensity use. I suggest that the rows have a much tighter chronology, each row representing only a year or so of activity. If that is so, then there must be gaps of several years in the record. Likewise, the late phase of activity, the Roman Iron Age pits, are so few that in *that* period, we cannot argue for a pit every year. Within the 1800 year chronology, we have periods of high and low intensity – but high intensity here may mean five or ten pits in one year, no more than that.

In addition to the pits, we have evidence of Late Bronze Age bronze casting (MAP). This combination of highly structured CSF and bronze casting is known from three other sites on Zeeland: Skamlebæk Radiostation, Holbæk Sportsby and Allerslev Golf. These are about 20-25 kilometres apart. Using these as our starting point, then, we can draw a net of Thiessen polygons and look for other assembly places. The Danish National database, *Fund og Fortidsminder*, allows us to identify candidates for more than half these, and, so doing, identify nearly half the known sites with CSF-fields and -rows as assembly places. *This model allows us to account for more than half the known "big" CSF sites on Zeeland, and to identify more than half the proposed gathering places archaeologically*. (BLANK)

Moving on, let me talk briefly about C14 and more about how we selected features for sampling:

We have obtained a total of 104 dates from 52 pits, two dates for each feature. Obviously, there are trade-offs involved: we could have had more dates for each pit and dated fewer, or vice versa, but more dates would also require selecting more samples and accepting less suitable samples. We elected to do it this way.

We selected features to capture the full variability of the data, that is, long medium and short pits, round, oval and elongated pits, pits without stones, with only smaller stones and with big stones,

and so on. *Let me stress that I do not believe we would have captured the full chronology without this selection.* We also selected pits to achieve a good spatial spread, and that last, at least, paid off:

As this MAP shows, we have a good, clear horizontal stratigraphy on the site. I would not dare guess how far this can travel, but on this one site, at least, I would now dare to guestimate the date of most of the pits based on the 52 we have dated.

The earliest phase, yellow, dates to roughly period III-IV of the Nordic Bronze Age. These pits are found on the hilltop, and at the eastern end of the massive belt. The rest of the massive belt and the smaller belt in the north, near the bottom of the hill are Late Bronze Age, period V-VI, green. As you may be able to see, the LBA pits make up the largest single number on site. Halfway down the slope we have a small cluster of magenta signature, Pre-Roman Iron Age, and scattered across the lower hill, north of these, three dark blue signatures, Roman Iron Age or even Migration Period.

During different periods, people used different sections of the hillside. However, they used the same hillside for an unbroken period of some 1100 years and repeatedly over more than 1500 years.

All this information mainly contributes to understanding this one site.

This chronology also allows a description of spatiality within the clusters: our early pits are randomly scattered, about four or five meters apart; the LBA pits are aligned in neat rows; the PRIA pits are randomly distributed, but very close, one to two meters apart; and the RIA pits are spread very widely, tens of meters apart. This is a description that might actually travel. At the very least, I think future research should look into the possibility that pits of different dates might have different scatters and placements in the topography.

This information may be relevant for other sites as well as this one.

As for other variation, my findings range from the banal to the inconclusive (CORPLOT). Our dates support the basic claims made more than a decade ago, that short pits are *generally* earlier, longer pits later, that round pits are *generally* early, oval later, and elongated and rectangular even later, and so on. *However* the most important observation here is that *these are tendencies with a lot of overlap*. Not every round pit is earlier than every oval pit, not every short pit earlier than every long pit. I would not dare to date any individual pit on morphology alone. Relations between date and stone content seem even less informative.

Some of this blurring may be due to our data collection: as I shall discuss momentarily, categories like “round” and “oval” are somewhat subjective. However, I suspect that the blurring is real. After all, prehistoric people probably did not set out to make a round or an oval pit, specifically. Rather, these differences may reflect changing digging tools or changing social needs. Thus, we cannot expect a precise single date of transition, no matter how carefully we register our observations.

With this in mind, let me turn to data collection:

When my colleague, Jonas Sigurdsson, finished the trial excavation, he concluded that we were going to dig a large number of CFS on this site. Consequently, he turned to literature and drew up a data collection sheet (SHEET), partly based on earlier versions we had used on previous digs. I had little time to work with this data until after the excavation was finished. At that time, only, did I begin to see the potentials and the limits of our strategy.

Any data collection may be plagued by *redundancies* and *omissions*. This particular sheet also collects *heterogenous data*. Finally, as already mentioned, some data is more *objective*, some more *subjective*.

Redundancies are data already collected elsewhere. For example, data on feature length and depth are also collected on section drawings. In fact, the only truly unique data on this sheet is data about the stones. If you are designing sheets for future data collection, focusing on more unique data might be a good way forward.

Omissions are obviously a bigger problem: any data never collected at all is lost, and any data not collected systematically is difficult to analyse. In our case, the sheet does not include data on reddened subsoil. We did try to collect this anyway, but I can see we did not do so systematically enough. Specifically, we did not systematically register which features *did not* have this characteristic. In consequence, I can draw up a list of features where it was definitely observed, but not any list of features where it was definitely absent. Nor do we have systematic data on how far it extended – bottom only, one side, both sides… Arguably, we should also have collected data on feature width. This might allow a more precise typology of surface shapes.

Heterogeneity makes analysis difficult. Most of our data is multiple-choice format. A little is pure numbers. Some is pure text. Obviously, you can do different things with these – calculate averages for pure number, for example. Just as importantly, pure numbers easily overshadow everything else, just because you can do so much with them. Yet, I must caution, that this does not mean that they are necessarily more important than other information. To some limited extend we can circumnavigate these problems and reduce everything to multiple-choice format: for example, we can divide all the measured lengths into three intervals, short, medium and long. So doing, we lose detail but gain overview.

The really unique data on the sheet is all about stones, so let me dwell here a bit. This is where I see potential for better, future research. (CLOSEUP: STONES)

We registered stones on two independent axes of variation, size and breakdown. In both cases, we registered simply presence/absence, not relative or absolute numbers. Breakdown proved a problem: excavators disagreed about what these descriptions actually meant, and I wonder how far any of them relate directly to past activities – I suspect that e.g. disintegrated stones are broken down as much because of their geological composition as because of human action. My point is that we need more experimental work and more consultancy with geologists to really do top-quality data collection, and that we need better photographic guides to hand out to field assistants to make sure that registration is both meaningful and uniform. We may also consider making size and breakdown *co-dependent* variables, and registering this information in a table format.

I will not hide my frustrations: I have been unable to say very much about stone breakdown, despite having a data set of more than 1000 posts (VIOLÌN PLOTS). Either this data is genuinely meaningless, or else our data collection is insufficient. I do believe somebody someday should be able to do something with it, but I think we need a better strategy, and a firmer idea about what stone breakdown actually says about past activities.

Looking just at stone size, our sheets allowed a potential 64 possible combinations, two to the power of six. In reality, less than half of these occur, and some are far more common than others. A fairly simple analysis allows us to reclassify all pits into just three types: with no stones bigger than 5 cms, with fist sized stones, and with stones bigger than 20 cms (NETWORK GRAPH, TREE).

So far, I have not spotted any chronology in stone selection, but I am not done analyzing this data set yet.

As part on my analysis, I looked at correlations between all these variables. Unfortunately, nearly all findings are banal: we can see that round pits are often short and round in section, that oval pits tend to be longer and more often flat-bottomed, and that elongated pits are even longer and flatter (CORPLOT). No surprises there. We can see that pits with intact stones often contain bigger stones, pits with more broken stones smaller fragments. No surprises there, either. What is mildly interesting is that pit morphology and stone content shows almost no correlations at all. I suggest that *pit shape reflects the scale of activity, stone selection reflects the nature of the activity*, and that

the two did not really co-vary at all. Chronologically, there are weak tendencies in pit morphology, to bigger pits over time, but apparently not in stone selection.

As part of this excavation, we also performed a pilot experiment. In brief, we excavated the smaller northern area first. Then, I looked at the data and decided that the CFSs there formed three large types: small round, medium round and medium oval. We selected the five deepest pits in each category, and re-excavated them, keeping loam and black fill apart, sieving and weighing everything, and sorting the stones twice, by size and by breakdown (PHOTO).

Again, most the findings are banal (GRAPHS). More importantly, this turned out to be very slow work: re-excavating each pit took about two whole person days, some 15 hours of work. We had a big excavation and a big budget, but I cannot see how we could accommodate much more than fifteen pits – yet this dataset proved so small that only the most obvious tendencies can really be discerned. This, then, is an economic dilemma: high quality data is expensive, meaning that we can only collect it by sacrificing quantity, while low quality data is much cheaper to collect, but much less informative. There are possible solutions, including unpaid or under-paid labour, but those entail their own problems, ethical and otherwise.

So, a few words on economy: basic data collection with the standardized sheet took a few minutes or each feature, maybe a total of two to three person-weeks of work. Data entry took something like four person-weeks. Compare this to the 48 or so person-*months* spend digging the whole site – data collection took up only a few percent of total work time. The pilot project, on the other hand, took some 30 person days – six person weeks – to investigate 15 pits, so around 1% of the total workload on just 1.5 *per mille* of the CFSs, less than 0.5 per mille of the 3500 features onsite. My point is simply that experimenting with new excavation techniques requires solid budgets and early reflection and assessment on whether this is the best way to use our resources.

So, let me sum up a few main points:

We need to locate sites in much larger landscapes. This means topography, but also large area excavations. At Alpegård, we are very fortunate that almost the whole hilltop has now been investigated.

I suspect that long chronologies are more common than we have realised. If we had obtained only a handful of dates, I doubt we could have argued for the unbroken, 1800 year chronology of this site. When we do get to dig these massive sites, I think we must insist on budgets for large scale C14, and we must embrace statistical sampling strategies.

In general, fine grain data-collection can always be reduced in complexity later, but not vice versa.

Pilot projects are a good way to test new practical approaches, but we need to allow room for failure to grow: out pilot project largely concluded that nobody need to do this particular experiment again.

And one more thing: when we submitted charcoal for sample selection, our botanist pointed out that two of those pits contained significant numbers of charred grain and some seeds of whitethorn berries. I suggested, and she embraced, the idea that these were brewing pits, as described by Odgaard and Henriksen. Since then, I have identified one more. Had we had soil samples from every pit, we might have been able to identify even more of these. In any case, they are surprisingly big, with very few stones, but we could never have found them from statistics alone. Here, as always, some of the best results are entirely unforeseen. We cannot plan for everything.

In conclusion, then: we can do cheap, useful, systematic data collection with tools we already have. We need better photographic guides for stone breakdown, and better data for what this breakdown

actually means. We should do some pilot experiments, but also assess these critically. And, perhaps most importantly, for large sites we need both an intelligent sampling strategy and a large number of C14 dates to understand the real complexity of the sites. We all need to insist, and the Board of Culture needs to accept that you cannot date a thousand pits with ten samples.