According to Issue #9;

> *Refs that show models where: models infer parameters (e.g., optimal chill) from other data or studies, then fit only certain parameters of their models to a dataset at hand. I have Richardson 1974, but there MUST be many more.*

1. Can't find richardson1974 right now, but since chillingms.tex says:

> *Some of the early models from peaches, which underlie many of today's models, used experimental data of percent leaf or flower burst as evidence that chilling has been met then attempted to identify the range of temperatures where chilling accumulates \citep{erez1971}. Taking these estimated temperatures, they then used field observations of percent flowerburst for plants across a wide range of climates---including those well outside the natural range where percent flower burst in many years was low---to estimate the total chilling needed for different cultivars \citep{richardson1974}.*

And also this:

> *One way to increase the amount of data used to estimate chilling models would be to include both experimental and observational data together in one model, but this has never been done to our knowledge. Instead, models continue to infer parameters from other data or studies, then fit only certain parameters of their models to a dataset at hand \citep{richardson1974}. While conditions in observational and experimental are often different---for example, many experiments apply cold temperatures in the dark, while photoperiod shifts each day in observational data---continually fitting the resulting data separately slows progress towards a coherent model and contributes to the increasing diversity of proposed models today.*

2. So if X paper took the data from the Erez *et al.* series of papers on growing peaches but in some other country instead (like from the Mediterranean to the US, which I think is exactly what happened, like in the Utah Model??) and built a model out of this (like, chill units or chill time etc.). Then, as in richardson1974; "taking these estimated temperatures…used field observations of percent flower burst…to estimate the total chilling needed for different cultivars", where:
    a. "estimated temperatures" = the model built in a prior paper, in this case erez1971
    b. "Field observations" = current paper or a prior paper's empirical data
    c. "Estimate the total chilling" = applying said model from a prior paper to a whole new study system, then generating their own model outcomes

3. Any papers that come to mind with this kind of system?
    a. Overcash & Campbell (1955): explicit use of Weinberger (1950)'s peach cultivar chilling requirement model;

> *The two varieties chosen were Sunhigh with a short chilling requirement; and Redhaven, with a long chilling requirement. Weinberger…states that Sunhigh needs 750 hours of chilling at 45 degrees F or lower to break the rest period…Redhaven requires 850 hours for flower buds and 950 hours for leaf buds…at Fort Valley, Georgia.*

This paper used intermittent warming to show that it could disrupt chilling accumulation (for lack of a better word, given the understanding of chilling at the time);

> *Both 750 and 950 hours of intermittent chilling were not totally effective in breaking the rest of either Redhaven or Sunhigh, because some of the symptoms of prolonged dormancy are displayed.*

And fortunately, Overcash & Campbell (1955) also links to Bennett (1950) which did a similar experiment with hardy pear, and indeed this paper is the one which sparked the whole optimal chill temp clown fiesta. It also links to Weinberger (1954) which used the 'Sullivan Elberta' peach cultivar and found similar results of intermittent warming. There's also;

> *The effectiveness of high air temperatures and consequently higher twig temperatures on bright, sunny days in counteracting a certain amount of the chilling influence of lower temperatures is offered as a partial explanation of prolonged dormancy in Texas by Denman.* (referring to Denman (1954))

b. Fuchigami et al. (1982) looks promising because they outline in intense detail the definitions and parameters of their own model (the degree growth stages model) and, in each section, have papers that are relevant or support the development of this model (and they credit some of its structure to Sarvas (1973; 1974). This one is long though and needs some digesting…but it seems promising for a later bullet point in this issue?

c. The model of apple bud development in Landsberg (1974) is built basically from scratch using empirical data, so it may be worthwhile to scan for papers that cite Landsberg (1974) in the creation of their own models or when they apply it to another study system

As a side note, Rageau *et al.* (1998) talks about 4 previously established conditions (things like temperature ranges, durations, etc.) and tests them to check their validity; maybe only tangentially related to the issue at hand.

> *Original or very early papers that talk about using cuttings to determine ecodormancy -- possibly showing that these terms and ideas evolved around the same time.*

1. Of the original/early papers, which ones use cuttings? Most evidently here is Erez & Lavee (1971) and Lamb (1948);

> From Erez & Lavee (1971), *Excised shoots: Mature shoots 40-50 cm long, of the last season, were collected in the orchard.*
> And from Lamb (1948)*, In this study a large number of canes were cut October 14 and stored with their butts in water at 37.4 degrees F and 26.6 degrees F.*

2. Now to see if they are using these cuttings to determine ecodormancy; quick refresher than **eco**dormancy refers to the state at which the dormant buds are able to burst and rely entirely on external factors, not internal ones like endogenous hormone ratios

> *Fig. 1. Effects of controlled temperature on breaking the rest of Latham raspberries. 1946.*

The paper doesn't explicitly state ecodormancy as one of the things it's testing, but the figure does sort of reveal it; that at a certain time of accumulating sufficient hypothetical chilling, the time taken for budburst to

occur appears to flatline and does not decrease further, providing an implicit graphical depiction of ecodormancy

And to be honest, it seems like Erez & Lavee don't really bring up even the idea of ecodormancy much, so not worth using them as a reference for this.

3. Can we get ecodormancy from original/early papers through Web of Science instead? In Web of Science, the search term "ecodorman*" yields the year 1987 as the first instance in the search list where a definition of the term is given (although it's highly likely that, due to Web of Science not covering every single publication in the world, ecodormancy as an idea probably emerged a bit earlier; nevertheless, a good starting point). The second oldest result gives Inoue (1990), a paper on potted satsumas exposed to chilling conditions.

   In the search term ((cutting* OR scion* OR excise*) AND (ecodorman*)) we get 20 results (though most don't seem that relevant, primarily experiments on adventitious rooting in winegrapes or molecular studies) with the earliest being 1993 (which is Tamura *et al.*) and a total of 20 publications. I will add these to refs/refsReviewedJNgo shortly!

   By the time Tamura *et al.* (1993) came out, ecodormancy was probably already an idea floating around, considering that the term is defined in that Web of Science search hit in 1987, but this was only 6 years prior so it's probably within reason to assume that people were thinking about this entire concept of cuttings/dormancy/budburst/chilling in general at this time period? And the first actual publication on an experiment was in 1990, just 3 years prior to Tamura *et al.*

> *Papers that discuss how well using % budburst and rapid leaf out at high temperatures for non-fruit trees actually works as a determinant of endo-dormancy.*

Which of the papers currently in the refs list are not about fruit trees, or any agricultural-oriented species in general? Which papers talk about endodormancy explicitly? A lot of papers will touch upon how budburst is a sign of dormancy release, but I'm not sure if they discuss the validity of this approach…keep looking!

It would be easy to wonder if budburst and leaf out were valid determinants for **eco**dormancy considering these two are conspicuous phenological events, and it might fully be the case that demarcations of endodormancy are purely at the molecular scale.

1. Zhang *et al.* (2023) might offer some insight;

> *Nevertheless, whichever definition of the Δt is used, the classical approach described above is essentially a black-box one, for it makes use of no observations or measurements of the growth-arresting physiological and molecular conditions or their removal inside the buds per se. In other words, the invisible endodormancy release that has taken place in the buds under the chilling conditions by the time of the transfer to the forcing conditions is determined on the basis of the implications of the endodormancy release for the occurrence and timing of the visible bud burst in the forcing conditions.*

Here they directly address the fact that chilling time or other kinds of models that use chilling duration/temperature/etc. as input and days to budbreak as the output are essentially lumping endo- and ecodormancy together, since there's no molecular scale work done to show when the former ends and the latter begins. They also bring up an interesting point here, though it fits more into the first bullet point in the issue than this one;

> *However, though the experimental technique for determining the chilling response of endodormancy release is straightforward, it is laborious and time-consuming. This is evidently the reason why the chilling response of endodormancy has usually not been determined experimentally. Rather, the response has mostly been determined either* a priori*, or by means of inverse modelling, meaning that the values of all model parameters (not only the ones related to chilling effects) are estimated by fitting the model statistically to observational long-term phenological and air temperature records.*

Which made me think of the first point in the issue, with the whole recursive data to model to data to model problem going on. In a similar vein, Jewaria *et al.* (2021) gives an example of this problem of "inverse modelling";

> *It should be noted, however, that the model development by Chen et al. (2017) was based on the technique of inverse modelling, an approach in which process-based models are fitted to observational long-term phenological records. It has been known since the pioneering study by Hunter & Lechowicz (1992) that this method involves an exceptionally high degree of uncertainty and that biologically unrealistic models are quite often obtained with it (for a recent discussion, see Hänninen et al., 2019).*

Every time I look for papers that review endodormancy release and leafout timing it's always about fruits trees, whether its almonds or cherries or peaches or kiwi etc. BUT also, like Zhang *et al.* (2023), Fuchigami *et al.* (1982) reiterates from Sarvas (1973; 1974) that;

> *Attempts to relate growth stages to environmental factors have had limited precision because, to quote Sarvas, "there are no suitable, easily observable phases that can be accurately measured".*

Which is presumably why Fuchigami *et al.* (1982) then proceed to spend the entire paper defining certain growth stages in a quantitative way.

> *Examples where researchers developed falsifiable predictions from models of chilling*

This one I may not be able to answer confidently, in full honesty, but I can certainly try!