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Forum Article

Lichenometric dating: Science or pseudo-science?

Gerald Osborn [a,]*, Daniel McCarthy [b], Aline LaBrie [c], Randall Burke [d]

[a] Department of Geoscience, University of Calgary, Calgary T2N1N4, Canada
[b] Department of Earth Sciences, Brock University, St. Catharines, Ontario L2S 3A1, Canada
[c] Canadian Natural Resources Ltd., 2500, 855 2nd Street SW, Calgary, Alberta T2P4J8, Canada
[d] 1705, 700 9th Street SW, Calgary, Alberta T2P2B5, Canada

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ABSTRACT

The popular technique of estimating ages of deposits from sizes of lichens continues despite valid criticism, and without agreement on range of utility, treatment of error, and methods of measurement, sampling, and data handling. A major source of error is the assumption that the largest lichen(s) colonized soon after deposition and will survive indefinitely. Recent studies on lichen mortality suggest that this assumption is untenable. Meanwhile, the use of "growth curves" constructed from independently dated substrates is problematic for many reasons, but this has not prevented the publication of baseless claims of accuracy and ages that are extrapolated well beyond data. Experiments indicate that numeric lichenometric ages are not reliable, and in general do not advance the cause of Quaternary science. There are a few studies suggesting reliability, and indeed there may be cases where lichens and growth curves actually provide realistic numerical ages. But it cannot be foretold which lichen assemblages will provide good ages and which bad ages. The logical conclusion is that no assumption of good ages can be made, and that it is folly to assign numerical ages to a deposit on the basis of lichen sizes.

There must be some way out of here,
Said the joker to the thief;
There's too much confusion,
I can't get no relief……
[Bob Dylan]

Introduction

Since its conception by Beschel (1950) the measurement and interpretation of lichen sizes have become a very common technique with which to determine ages of deposits, most commonly moraines and bodies of colluvium. This technique is properly called lichenometric dating, as lichenometry is a broader term that may encompass measurements of lichens for other purposes. But almost all of the geoscience and biological literature uses lichenometry as a short form for lichenometric dating, as do we in this paper.

Overviews of the technique include those of Webber and Andrews (1973), Locke et al. (1979), Worsley (1981), Innes (1985), Osborn (1988), Noller and Locke (2000), McCarthy (2002, 2007, 2013), and Benedict (2009). According to Noller and Locke (2000) references to lichenometry in the total geosciences literature increased from an average of 5 per 100,000 papers in 1960 to 25 per 100,000 in 1995. O'Neal (2009, p. 316) states that "a survey of current literature illustrates the dominance of lichenometry in the reconstruction of late Holocene glacier chronologies worldwide". A Google search of "lichenometry" in 2012 returned 30,600 results.

Although the technique is popular, it has not escaped criticism. The most critical outlook is that of Jochimsen (1973), who concluded that highly variable lichen growth rates, resulting from dependence on substrate lithology and various microclimatic conditions, are not (and generally cannot) be accounted for in age studies. She also perceived problems with variable lichen ecesis intervals, ambiguous thallus morphology, and potential inheritance of the largest lichen(s). Worsley (1981) listed the same problems in different forms, and concluded that "the lichenometric dating method in its present form is conceptually unsatisfactory both with respect to its basic assumptions and to its method of field application". Elsewhere in the literature, Innes (1981) is critical of many of the lichenometric techniques recommended by Locke et al. (1979) in their Manual of Lichenometry, and Innes (1985) notes that "…the technique has been much abused…". McCarthy (2007) concluded that there is doubt as to how closely some lichenometric ages match the true ages of surfaces, and noted (McCarthy, 2013) that neither authors/editors nor readers ask or seek answers to basic questions arising from the method. Armstrong (2011) raised biological issues pertaining to development and growth of *Rhizocarpon* which impact lichenometry.

Despite the many published doubts, use of lichenometry continues, apparently oblivious to criticism. Its popularity stems no doubt from

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* Corresponding author.
E-mail address: osborn@ucalgary.ca (G. Osborn).

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apparent ease of application and general lack of expense. The result is a plethora of ages of glacial advances and landslides that may not have any basis in reality. In this paper we offer a strongly pessimistic perspective of the method as currently practiced, based on three claims: (1) A startling lack of agreement, and in fact debate, on range of utility, methods of measurement, data handling, and treatment of error suggests that lichenometric ages in general cannot be regarded as reliable; (2) There are theoretical reasons and observational data to show that crucial assumptions employed in lichenometry are not valid; (3) Experiments on reproducibility, and on lichenometric ages of independently dated deposits, generally come out negative.

The objective of this paper is not to review the whole subject, which has been done before, but to appraise the validity of numerical ages derived from lichen measurements. We do not assess other avenues of research that may depend on such measurements. It should be noted that some of the authors of this study have in earlier lives engaged in some of the practices criticized here, and favorably reviewed some of the papers criticized below.

Lack of agreement on practice

Range of utility

There are great differences of opinion as to what range of time may be addressed by the technique. Miller and Andrews (1972) suggest that *Rhizocarpon geographicum* may be a useful time indicator for deposits up to 7000 or 8000 yr old. Noller and Locke (2000) suggest that the range may extend beyond 9000 yr. Benedict (2009) suggests that the "theoretical dating range of maximum-diameter lichenometry may approach 10,000 years", but concludes that the practical limit of the method is closer to 4000–5000 yr. On the low end, Matthews and Trenbirth (2011) state that in Norway lichenometry has been most successful on surfaces dating from the last 500 yr, and Innes (1985) proposed 500 yr as the general useful limit. Because of lichen weathering at their site in Iceland, Gordon and Sharp (1983, p. 197), noted that "there may be limitations in extending the technique to dating surfaces more than about 100 to 150 yr old…". According to Matthews (1994), there is uncertainty over temporal range because of uncertainties regarding growth rates and the longevity of very old lichens.

The great variation in opinions suggests that (1) there is no definitively established range of lichenometry for any given species or any given environment, and (2) at a new site there is not much chance of knowing what the range will be for any given species, and whether or not the age of any given deposit is greater than that range.

Measurement

There is no general agreement on what to measure. Beschel (1961) suggested that any lichen with an oblong shape should be considered only in its shortest diameter. However, many have measured the longest axis of thalli that have roughly circular outlines (e.g., Calkin and Ellis, 1980; Denton and Karlén, 1973; Kirkbride and Dugmore, 2008), some have used the shortest axis (e.g., Dahms, 2001; Luckman, 1977; Osborn and Taylor, 1975) or the mean of the longest and shortest axes (Erikstad and Sollid, 1986) or the diameter of the largest circle that can fit inside the thallus (Gellatly, 1982; Locke et al., 1979). Some suggest that inclusion of coalesced thalli can be avoided by selecting only circular to nearly circular thalli (e.g., Lewis and Smith, 2004), but no parameters are given to define "nearly circular". Others measure the short axis "to avoid over estimating the size of less-than-circular lichens" (Dahms, 2001, p. 63). But, lateral expansion can be slowed by obstacles or microenvironment (Innes, 1985) so it can be argued (e.g., Locke et al., 1979) that longest axis best reflects growth under optimal conditions and the shortest axis underestimates growth potential. All users recognize that inclusion of coalesced thalli can potentially overestimate growth rate and underestimate age. However, as Bradwell (2010) notes, coalesced thalli can often go unrecognized. Overall, it seems that measurement of *any* size property is problematic, and every possibility has been criticized. Perhaps there is no good way to measure a lichen.

Sampling strategy

Number of lichens sampled

There are debates over whether the single largest lichen is the best indicator of substrate age (e.g., Calkin and Ellis, 1980; Webber and Andrews, 1973) or if an average of several thalli (usually 5 or 10) provides more accurate results by limiting the effects of anomalous thalli, (e.g., Innes, 1984; Matthews, 1974, 1975, 1977; Sikorski et al., 2009), or whether statistical treatment of data is best served by sampling hundreds or thousands of thalli.

Those who use the single largest lichen assume that the largest individual colonized the substrate first and is the best indicator of the age of the substrate. The argument against this method is that the single largest lichen on a deposit might somehow pre-date and survive the event to be dated and thus be older than the event. Use of the 5 largest lichens has also been suggested in light of the extreme variability seen in directly measured growth rates (Haworth et al., 1986).

Because mean long-term growth rates estimated from the five largest lichens will be slower and may not well correlate with that of the largest lichen, error is introduced when the two rates are used interchangeably. Calkin et al. (1998), for example, extended their growth curve for the Seward Peninsula by drawing it parallel to Denton and Karlén (1973) growth curve for the Swedish Lapland, based on a perceived similarity in macroclimates, although the Swedish Lapland curve was constructed using the single largest lichen while the Seward Peninsula curve uses the mean of the 5 largest lichens.

For other workers, 5 or 10 measurements on a deposit are not enough. McKinzey et al. (2004) conclude this approach is limited by the small data set that is not statistically robust. Many workers (e.g., Bradwell, 2004; Caseldine, 1991) employ a size-frequency approach which requires a large number of lichen measurements on a surface, often 200 or 500 or 1000, so that age estimations are based on a large data set. Some, (e.g., Bull, 2000; Matthews, 1975), average the largest individuals at a number of sites or stations. But there is little or no agreement on the appropriate statistical treatment of large data sets.

Opposed opinions are strong. Matthews (1974, p. 229) declares "Use of the [statistical] techniques outlined above provides a method which avoids the dubious practice of relying entirely upon the single largest lichen on each surface for dating purposes." The writer is thus in direct opposition to Webber and Andrews (1973), who state that "only the lichen thallus with the maximum diameter is an indicator of surface age, and that use of the single largest thallus is essential for effective use of lichenometry."

Kirkbride and Dugmore (2001) showed that variations in sampling strategy result in "poor repeatability" of lichenometric conclusions.

Search area

Where to search and how big an area to search are important considerations in lichenometry. Some workers keep search areas small to limit misinterpretations due to moraine morphology (e.g., Larocque and Smith, 2004), while others favor large search areas to increase the potential for finding the largest lichen(s) (e.g., Bradwell, 2009; Matthews, 1974; McCarthy, 2003). Innes (1985) recommends a search of the entire landform, while Locke et al. (1979) suggest that large fixed-area searches would allow for more comparable results between studies. This is another facet of lichenometry where disagreements in the methods applied create results that are not directly comparable between studies. If the effect of search area is as important as has been suggested (e.g., Innes, 1984), the results of a study where only the crests of moraines have been searched can hardly be compared to a study where an exhaustive search of the entire moraine has been performed. Many studies do not mention search area at all (e.g., Beget, 1994; Osborn, 1985) or offer useless terms for the search area such as "as much area as possible" (Calkin and Ellis, 1980, p. 251).

Further complications are introduced by conflicting opinions on how moraine morphology and the process of ice retreat can affect the size of lichens found in different locations on a moraine. Since the proximal side of a moraine may be covered by ice while the distal side is free, colonization could be deemed to start earlier on the distal side. This has led some researchers (e.g., Bradwell, 2004; Erikstad and Sollid, 1986; Matthews, 1974) to sample only the proximal sides of moraines, but this is not followed in the majority of studies and appears to be more popular in Europe than in North America.

Erikstad and Sollid (1986) avoided the bases of moraines for fear that anomalously large lichens, predating the moraines, could be found on boulders that had been pushed by the advancing ice but never incorporated into the glacier and destroyed. Allen and Smith (2007) and Sikorski et al. (2009) sampled only from the crests of moraines, without giving any rationale. But the opposite approach is suggested by considerations of moraine degradation. Hallet and Putkonen (1994) suggest that downslope movement of sediments over time will expose boulders on the crest of a moraine, such that lichens on the crests of moraines will be younger than the moraines. O'Neal (2006) presented evidence to show that crest lowering (and consequent exposure of initially buried boulders) of matrix-supported moraines is significant even on decadal to centennial time scales, and thinks the lower slopes of moraines are most likely to provide valid samples for age estimation. He concludes that slope degradation must be taken into account when constructing growth curves and dating landforms. As far as we know, no subsequent study has taken it into account.

Meanwhile, moraine crests are disregarded by Karlen and Black (2002) because of wind exposure… but Bull (1996) measured only lichens exposed to the wind.

Exclusions

Some workers exclude coalescing thalli (e.g., O'Neal and Schoenenberger, 2003), but most make no mention of the issue. Some avoid thalli located close to water or late lying snow (e.g., Andrews and Webber, 1969; Porter, 1981), exclude areas that appear to have been affected by snowkill (e.g., Refsnider and Brugger, 2007), reject areas of slope instability (e.g., Dahms, 2001; Young et al., 2009), and exclude thalli that appear to have been naturally fertilized (e.g., Osborn and Taylor, 1975). Exclusions are often based on subjective interpretations of present-day conditions that may not have existed when the lichens became established centuries earlier.

Summary

Every search strategy ever proposed has been criticized by other parties, for reasons that, whether or not they are valid, are at least logical. McCarthy (2007) noted that each new user of lichenometry seems to adopt a different approach to sampling and data interpretation. This may suggest that there is no good search strategy. The variety of approaches presents problems for inter-study comparisons, as well as reproducibility of lichenometric results. Excellent examples of non-reproducibility of results are described by Dąbski and Angiel (2010) and Angiel and Dąbski (2012).

Data handling

Practitioners who prefer large data sets traditionally use some form of size-frequency distribution. But there are many arguments over what this distribution looks like or should look like. Locke (1983, p. 419) in a pessimistic appraisal suggested that the lichen population structure *may* over time "follow a Poisson function from no thalli (all zeros), to a log/linear, to a truncated normal, to a positively skewed normal to a normal distribution." McCarthy (2007) suggests that the various statistical approaches are based on questionable assumptions regarding the statistical normality of the lichen population and its microenvironmental controls.

Chenet et al. (2010) reasons that because lichens measured on dated surfaces (for the local growth curve) and lichens measured on undated surfaces (the "unknown") are separated into two data sets for the analysis, separation of the two groups is statistically arbitrary because the distribution of lichen diameters comes from the same family of distribution. Furthermore, error is increased by the propagation of uncertainties from the first step to the second step. Chenet et al. (2010) follow the approach of Jomelli et al. (2007), suggesting that the Bayesian approach of fitting an extreme-value distribution to the largest lichen diameters offers the most reliable estimates of moraine age. But Dąbski (2010) questions the validity of the generalized extreme value method used by Chenet et al. (2010) in Finland because the results of the Finnish study do not agree with historical data. Meanwhile, Bradwell (2009, p. 61) reviews the proliferation of complex statistical treatments and asks "can statistical complexity and high precision in a 'geobotanical' dating technique, fraught with high degrees of environmental variability and inbuilt uncertainty, ever be scientifically valid?" The debate over complex statistics continues (Bradwell, 2010; Jomelli et al., 2010).

Treatment of error

Error is difficult or impossible to quantify in lichenometric studies due to the number of variables inherent in the method and the unvalidated nature of some of the assumptions. However, that does not prevent quantitative claims of error. Many papers make no mention of potential error(s) (e.g., Osborn and Taylor, 1975) but many do. Authors who provide some explanation of their error estimates generally consider only some of the potential sources of error. For example, Porter (1981) discusses possible error in lichen measurements and in determination of exposure or construction ages for control points, but does not consider variable rates of colonization or the possibility of different growth rates in different control-point environments. Lewis and Smith (2004) consider only colonization rates as possible sources of error.

Lack of agreement on, or consideration of, inherent error leads to variation in views of the meaning of lichenometric ages. Some authors regard them as very rough estimates (e.g., Nicholas and Butler, 1996) or minimum ages (e.g., Wiles et al., 2002) while others present them as absolute numerical ages (e.g., Porter, 1981). Some switch philosophies midway through a paper; for example, Larocque and Smith (2004) begin by conceding that their lichenometric ages are minima, but then treat them as absolute by comparing their lichenometrically derived ages of glacial advances to other dated glacial histories in the region. Allen and Smith (2007) first note that lichenometry provides only relative ages of termination of glacial activity, and then report lichenometric ages to single calendar years.

Some claims of small error ranges are based on alignment of control points on growth curves. For example, Larocque and Smith (2004) regressed their control-point data and using a 95% confidence interval estimated an error range of +45/−30 yr for a 150-yr-old surface. Their 95% confidence-interval envelope between 100 and 680 yr ago indeed suggests confidence, but is actually spurious, because the 500-yr linear portion of the curve is fixed by a single point, which could be anomalous due to species differences, unusual microclimate or snowkill, etc. They admit that this method of error estimation does not encompass all possible error and is not possible in remote settings with limited dating control, but suggest that it does provide "a useful statistical estimate".

Similarly, spurious foundations underlie claims by Naveau et al. (2007) and Jomelli et al. (2007) that use of extreme value theory in modeling maximum lichen diameters results in tight confidence intervals. For example, their oldest (and hence key) age for a lichen-bearing deposit is a calibrated ^{14}C date of AD 1630–1670 from a Charquini Glacier moraine, allegedly taken from a study by Gouze et al. (1986). But the latter paper contains no such ^{14}C date. The closest possibility is 220 ± 50 ^{14}C yr BP for peat in a moraine. But because of the radiocarbon plateau at ca. 200–100 ^{14}C yr BP, that age could translate into almost any calendar age between AD 1520 and 1950 (Calib 7.0 calibration program). Furthermore, the 220 ± 50 ^{14}C yr BP determination was apparently derived from a bulk sample of peat, which because it contains organic fractions of many different ages (e.g., Brock et al., 2011) may have little connection with the age of the lichen-bearing moraine.

Most treatment of error in the lichenometric literature consists of ad hoc estimates that have no particular quantitative foundation. Miller and Andrews (1972) apply an arbitrary error term to ages derived from their *R. geographicum* growth curve, such that a 155 mm lichen yields an age of 6000 ± 1200 yr and a 22 mm thallus yields an age of 310 ± 50 yr. They note "there is no statistical basis for the error term, rather it is a subjective interpretation of the reliability of the curve at present." Bickerton and Matthews (1992) claim, for no particular reason, ~10% accuracy on LIA timescales and ±20 yr for older moraines. Andrews and Barnett (1979) use ±15% as a "qualitative estimate" of error. In Alaska, Calkin and Ellis (1980, p. 257) adopted a "qualitative ±20% age accuracy", following Miller and Andrews (1972); this error range is assumed even on the extrapolated portion of their *R. geographicum* curve that extends for thousands of years beyond their oldest control point of 800 ± 90 ^{14}C yr BP. This was done despite the knowledge that (a) the 800-yr control lichen is only loosely constrained by a radiocarbon determination, (b) the analytical error range of one of the radiocarbon control points is greater than the stated growth-curve accuracy, and (c) the linear extrapolation of the growth curve beyond the 800-yr control point is not based on any data from the authors' study area, but rather from "measurements of *R. geographicum* obtained from other parts of the world…" (Calkin and Ellis, 1980, p. 257). The ±20% error range has been adopted in most subsequent Alaskan lichenometry papers.

Baseless error estimates have a way of becoming accepted doctrine as they pass from one paper to another, or even from the beginning of a paper to the end of a paper. Young et al. (2009), for example, begin by considering the now-entrenched error estimate of Calkin and Ellis (1980) with the statement "The uncertainty associated with lichenometric ages is typically cited at about ±20%" (p. 683), and later in the paper accept it as truth: "Taking into account the ±20% uncertainty of lichenometric ages, the methods produce ages that overlap" (Young et al., 2009, p. 686).

Summary

Lack of agreement on lichenometric practice, and criticism of all methods of data collection and handling, render lichenometric results suspect. Statements of error, when presented at all, are generally incomplete or spurious.

Why lichenometric dates are unreliable

Ecological considerations

Lichenometry rests on several assumptions, rarely articulated, that are unverified or patently incorrect. The key assumption is that the largest lichen(s) on a deposit colonized soon after deposition and continued to grow through the interval between colonization and observation. For example, Calkin and Ellis (1980, p. 247) state "… the diameter of the largest lichen thallus is proportional to the age of the surface in question, if it may be assumed that the lichen is one that colonized shortly after… initial surface formation and that the lifespan of the species utilized is greater than the age of the surface." These things are assumed in almost all lichenometry studies, but evidence to support them is lacking.

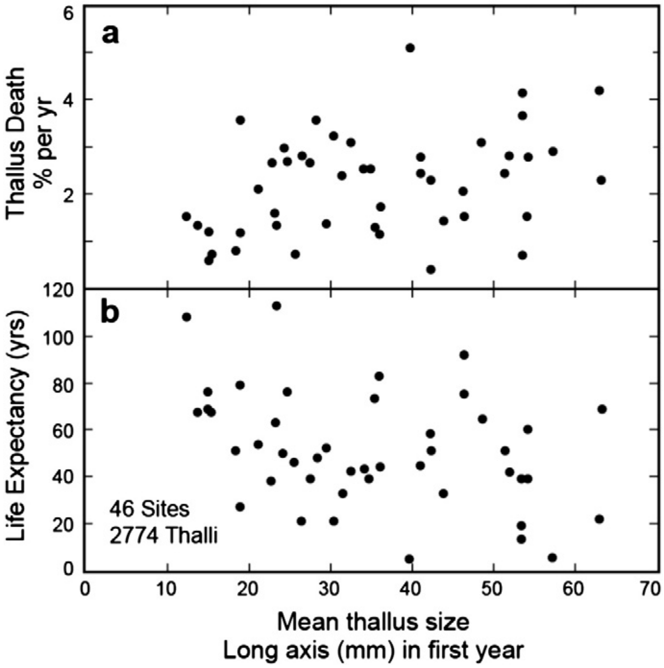

Figure 1. Death rate and life expectancy data reported by Trenbirth and Matthews (2010, Table 1). The observations were done over a 19 yr period at 46 sites in 17 glacier forefields in southern Norway. The thallus size data are unvalidated. Graph a shows a wide scatter of death rates and a weak correlation ($r = 0.36$) between mean thallus size and mean annual death rate. Graph b plots the life expectancy estimated by dividing sample size at a site by the observed death rate at that site.

Insight into the accuracy of these assumptions can be gleaned from a few long-term studies. For example, mortality rates can be crudely estimated based on a sample of 2774 *Rhizocarpon* agg. thalli that were marked and measured over a 19 yr period at 17 glacier forefields in southern Norway (Trenbirth and Matthews, 2010). Comparison of sample sizes at the start and end of the 19-yr interval suggests that mean annual mortality rates ranged from 0.38 to 5.09% yr^{-1} (Fig. 1a). These Norwegian rates broadly resemble the 0.41 to 3.66% annual dieoff found by tracking 123 marked *R. geographicum* thalli for 16 yr at the Illecillewaet Glacier, Canada (D. McCarthy, Brock University, unpublished data).

Much higher mortality rates have been found in juvenile thalli (e.g., 14% annual mortality in a sample of approximately 350 tiny thalli less than 1 mm^2 in area; T. Bukovics and D. McCarthy, Brock University, unpublished data). Clearly, *Rhizocarpon* communities are dynamic and *R. geographicum* lifespans may be much shorter than commonly assumed. For example, the time required for the Norwegian population to drop to zero (survivorship) can be estimated by dividing mean death rates per site by the number of thalli examined at that site (Fig. 1b). Using this approach and reasoning that a thallus could take about 40 yr to reach a diameter of 21 mm (0.5 mm yr^{-1} radial growth), we estimate a 160-yr life expectancy for a 21 mm thallus. Closer examination of the scatter of points (Fig. 1b) shows that most *R. geographicum* thalli died in a few decades and mean annual death rates were very weakly correlated (r = 0.36) with thallus size. These observations do not well support three of the most important lichenometric assumptions: i) the largest thalli began growth soon after the surface became stable and exposed, ii) the original colonists are long lived, and iii) the original colonists are the largest in the population. Since a healthy looking thallus may be dead, but still attached to a substrate, these mortality estimates are conservative minima (Fig. 2). Higher death rates might be expected in populations that have parasitic mycobionts, are subject to abrasion, are buried under prolonged ice and snow, or are

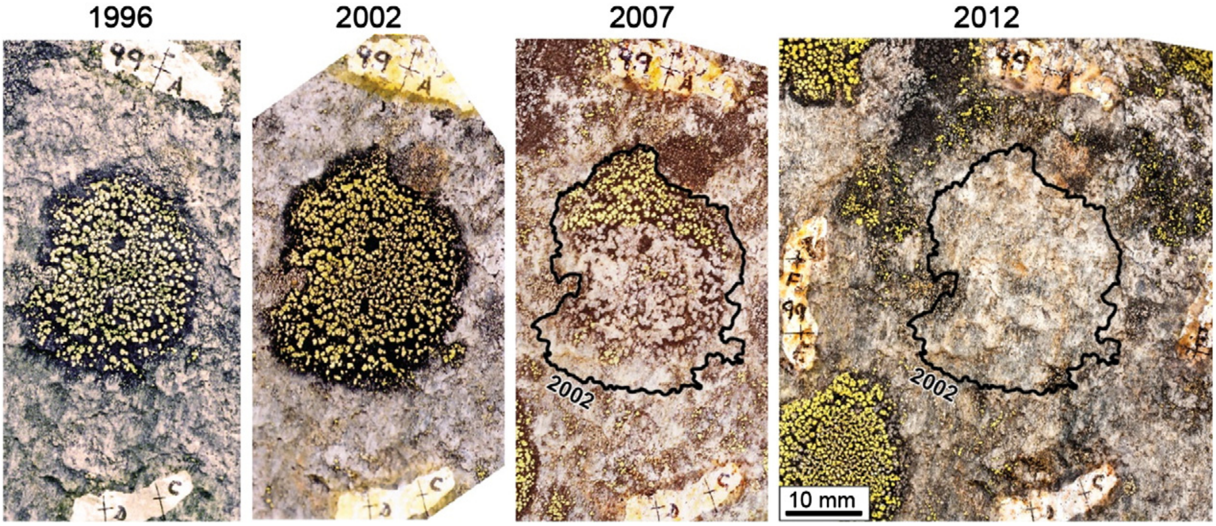

Figure 2. Death of an *R. geographicum* agg. thallus at the Illecillewaet Glacier lichen monitoring site. First photographed in the summer of 1996 (McCarthy, 2003), this thallus looked healthy and was growing in the summer of 2002. A sketch line shows the outline of the thallus in 2002. Dieback was well developed by the summer of 2007 and the thallus was no longer on the rock in 2012. Often, *R. geographicum* agg. thalli at this site have disappeared from the rock within two years of showing dieback, but sometimes thalli experiencing dieback have recovered and regrown.

exposed to air pollution or acid rain (e.g., due to volcanic eruptions). Unfortunately thallus size data offer no proof that a site still has most of its original colonizers; a surface can be much older than the lichens it supports. Closely limiting ages for old surfaces are especially suspect since the likelihood of at least one major ecological disturbance is expected to increase, and the stability of the species assemblage to change, as the succession proceeds. If the Norwegian lichen survivorship data are broadly representative (Fig. 1), lichenometric ages >160 yr old should be viewed as highly suspect.

Recently, explicit demographic modeling has emerged as an ecologically defensible approach to lichenometry. In this approach surface ages are estimated by assuming and/or indirectly measuring lichen colonization, mortality and growth rates. For example, Loso and Doak (2006) constructed demographic models that could explain size-frequency distributions of lichens growing on well-dated surfaces. They adopted a 2–3% annual mortality rate for *R. geographicum* agg. and *Pseudephebe pubescens* (L.) M. Choisy. This modeled rate is smaller than the maximum observed rates in the Norwegian study, but the implications are the same: the probability of finding an early colonizer decreases as surfaces get older, and on any particular rock surface one cannot be sure that the largest lichen is an early colonizer.

The impact of thallus crowding on lichenometric dating is also an important limitation that has largely escaped critical review. Thallus coalescence is common in young *Rhizocarpon* thalli (e.g., Asta and Letrouit-Galinou, 1995) and mosaics are often seen in century old *Rhizocarpon* communities. Consequently, at some sites, natality and crowding may set an upper size limit on the largest thallus. Jettestuen et al. (2010) addressed the issue with a simulation model in which a flat surface was randomly “seeded” with thallus initials. Their conclusion was that lichenometry on densely colonized sites will “systematically underestimate” surface age because it fails to compensate for thallus contacts that will block lateral growth. Thallus contact might also result in a “truce” condition, or one thallus could overgrow and remove another (Pentecost, 1980). This point has been explored by a few other authors (e.g., Loso and Doak, 2006; McCarthy, 1999) and may be the “methodological artifact” alluded to by Clayden et al. (2004, p. 379).

Thallus size distributions are constantly in flux; they are a complex function of mortality, natality, time elapsed, and unknown competitive/successional changes. Given this complexity and unknown variability of growth and thallus shape changes in a lichen community, lichenometric ages estimated in one field season need not closely match ages estimated in a subsequent investigation. Thus it is unclear if reproducibility or the lack thereof occurs by chance, reflects changes in community dynamics, and validates or invalidates one or another methodology. Calibration of accurate models that track mortality, natality and the areal growth of *Rhizocarpon* thalli requires repeated long-term direct measurement. This has not yet been done.

Statistical analyses and manipulations also involve the use of questionable assumptions. For example, perhaps it is time to question whether pseudoreplication (Hurlbert, 1984) due to temporally and spatially autocorrelated factors (e.g., rock weathering, autecological effects and microclimatic changes) is at play. Pseudoreplication exists when climatic effects (the “treatment”) are not identical at all microsites (e.g., due to microenvironmental differences, aspect, and relief and differences in solar input); then the experimental units (lichens in plots that received uneven environmental treatments) are not true replicates and it is not possible to statistically distinguish between treatment (time) and effect (growth). Presumably, this would be more of a concern on older surfaces where interplays between weathering, community density, lichen species richness and late successional change are especially well developed.

Growth curves

The continued mischaracterization of a lichen thallus as an “individual” and ignorance of biology has helped to foster the misconception that thallus size-surface age scatterplots are “growth curves” that depict the growth trajectory of some ideal thallus (e.g., the oldest thallus at a site). True growth curves use repeated measurement to track the areal growth or biomass changes of marked individuals. When measurement accuracy is validated and measures can be statistically shown to well represent changes all around the thallus margin, these data can be used to characterize growth. Those data are not equivalent to indirectly calibrated "growth curves" that report thallus sizes in lichen communities that are constantly experiencing losses due to mortality. A scatterplot that matches lichen sizes to the known age of the substrate surface is an ambiguous data set that mixes growth rates with thallus sizes that are a by-product of ongoing mortality. Even disregarding the inconvenient issue of mortality, indirect establishment of growth curves is replete with problems:

(1) *Independently determined ages of control points may not be robust, or may be unrelated to lichen size.* For example, tree-ring dates have been used in some cases as control points (e.g., Bull, 1996; McCarthy, 2003; McCarthy and Smith, 1995) but ages determined from trees are not necessarily any more reliable than ages determined from lichens; some of the same assumptions are employed. In fact, Porter (1981) concluded that in his area lichenometric ages were more accurate than ages based on tree rings. Use of even the most accurately dated rock surfaces (gravestones) for control points is replete with problems. For example, Innes (1983a) showed that in highland Scotland (a) there is considerable variability of size/age relationships from site to site, (b) the thalli growing on any particular gravestone do not necessarily represent the fastest growth rate of lichens at that site, and (c) many apparently suitable gravestones have no lichens at all on them.

(2) *Environments of control points may be, and probably are, variable.* Environmental factors affect the growth and survival of lichens (e.g., Beschel, 1961), but independently dated surfaces of different ages generally cannot be found in a single environment in a single small region. Lichen growth curves calibrated in one environment may not be appropriate in another.

(3) *Environments are not consistent over time*. The assumption that temperature and precipitation regimes have remained relatively constant since the Little Ice Age or longer (e.g., Hansen, 2008) is not secure.

(4) *Different species have different growth rates*. Lumping different species together into some kind of *Rhizocarpon* aggregate, most likely combines control samples that have different growth rates. Each species mix may have a different growth rate that may be site and community specific.

(5) *Many curves are extrapolated beyond data*. Usually there are many control points for young surfaces and few points for older surfaces (e.g., Calkin and Ellis, 1980; Denton and Karlén, 1973). When curves are extrapolated, sometimes by thousands of years as in the examples above, any errors in control points are greatly magnified. Extrapolation involves unsubstantiated assumptions about constancy of growth rates. Caseldine (1987) believes ages based on curve extrapolations should be regarded only as minima, but the problems mentioned here can result in either overestimation or underestimation of ages, depending on the circumstances. Curves with only one point/thallus constraining older portions of the curve (e.g., Beget, 1994) are also questionable.

(6) *Growth-curve shapes are poorly understood.* A common conclusion over the last few decades is that growth curves of long-lived lichens include an initial "great growth" period and a subsequent period of slow linear growth of unknown duration. The "great growth" period is often formalized as an exponential mathematical function (e.g., Porter, 1981) or may be described with a second-order polynomial function (e.g., Sikorski et al., 2009). The equation in such cases is used to date the unknown surfaces. Differences in the model chosen will of course yield differences in lichenometric ages. Direct measurement studies (e.g., Armstrong, 2005; Trenbirth and Matthews, 2010) have not resolved uncertainty about appropriate growth-rate models and there is no consensus regarding the shape of the growth curve, particularly in older thalli (e.g., Armstrong and Bradwell, 2010; Bradwell and Armstrong, 2007; Matthews and Trenbirth, 2011). Loso and Doak (2006) demonstrated that if small, young thalli grow relatively slowly, as they believe, an apparent early great-growth phase is merely a consequence of lichen mortality, not of varying growth rates. However, little is known about mortality rates and McCarthy and Henry (2012) showed that very small, young *R. geographicum* agg. thalli can grow extremely fast, even doubling in size in a single year. Clayden et al. (2004, p. 379) speculated that "great growth" is "probably a methodological artifact." The jury remains out.

Any lichenometric approach that equates lichen size-distributions to time elapsed must assume a great deal about how lichen communities evolve. Despite all the uncertainty, we see that "growth curves" developed at low-elevation sites have been used to estimate the age of deposits at high altitude glacial forefields (e.g., Lewis and Smith, 2004), a growth curve from the Colorado Front Range has been used to date rock glaciers in the La Sal Mountains of eastern Utah (Nicholas and Butler, 1996), and deposits in the Brooks Range of Alaska have been dated using an extrapolated curve based on data from "other parts of the world" (Calkin and Ellis, 1980). In a few cases long-distance transference of growth curves has been shown by later work to be invalid (e.g., Beget, 1994). Wiles et al. (2010) note that many control points for growth curves are based on partially forested or settled areas that are removed in both space and elevation from moraines that are being dated. Mortality is also a factor; little trust should be placed in lichenometric ages if they are based on "growth curves" or size distributions "calibrated" on surfaces that may be older than the lifespan of most lichens in a population (e.g., >160 yr for *R. geographicum* agg).

Measurement and sampling

Unlike scientists in other fields (e.g., geochemistry in general or radiocarbon dating in particular), users of lichenometry do not evaluate measurement accuracy and precision through the use of certified reference materials or conduct replication experiments. In rare cases an author will report made-at-same-time replicate measures as a way of demonstrating precision and replication (e.g., Bull and Brandon, 1998); however, double-blind testing has seldom been done (e.g., Innes, 1985) to evaluate operator bias or test search accuracy and precision at a control site. Bull and Brandon (1998) may have been the only study to test and report on the possible link between measurement error and thallus size. Increasingly, workers are using calipers to measure lichens. Most report the manufacturer's estimated instrumental error but do not consider measurement error (e.g., Savoskul, 1997) and/or claim a specific range of measurement accuracy (e.g., within 0.1 mm: Karlen and Black, 2002). Despite this, it is intuitively obvious that small errors in the measurement of slow growing lichens will cause large errors in age estimation. This general lack of scientific rigor would not be acceptable in most other fields.

The issue of variable environments considered earlier raises questions about statistical independence of "replicate" samples collected on a deposit. Implicit in approaches that use random samples/quadrats on a morainal ridge is the assumption that any factor that disrupts the growth and survival of thalli at one site is equally applied at all other sites. If mortality rates are not the same in the 19 glacier forefields reported in Fig. 1, it is not safe to assume that mortality rates would be the same all over a set of moraines. If mortality rates vary on a deposit due to differential disturbance or microenvironmental differences, the resulting samples did not all receive equal treatment and are pseudoreplicates. Consequently, "pseudo-confidence intervals" generated by the use of these samples will be too small and there is an increased risk of Type 1 error (false rejection of a true null hypothesis) (Hurlbert, 1984).

Identification of species in the field

Some have claimed that identification to species level is not necessary because *R. geographicum* is the fastest growing species and will therefore be preferentially selected as the largest thallus regardless of the presence of other species (e.g., Benedict, 1967). This belief was weakly contested by anecdotal observations, which led Luckman and Osborn (1979) to claim that *Rhizocarpon macrosporum* grows faster than *R. geographicum*. Innes (1983b) lent support to this belief by describing species-specific differences in thallus size on a few moraines in Norway. More contrary evidence was reported by John (1989) who concluded that the largest *R. geographicum* thalli on a rockslide in Alberta were smaller than thalli of the other two *Rhizocarpon* species (*superficiale* and *eupetraeum*). Unfortunately, that report did not provide details of the search and measurement strategy for this very large deposit and comparative size data for different parts of the landslide were not reported. Other plausible interpretations could be suggested (e.g., differential mortality and/or crowding) but were not entertained.

After roughly half a century of use, the fact remains that we still do not have a directly measured growth rate data set to show that there are/are not statistically significant differences in the growth rates of *Rhizocarpon* subspecies *Rhizocarpon* and thalli in the *Rhizocarpon* section *Alpicola*. Until such data are published, we can all believe what we wish.

Further complicating the issue of misidentification and associated errors is the almost complete lack of knowledge about the ecological characteristics and competitiveness of *Rhizocarpon* section *Alpicola* and *Rhizocarpon* section *Rhizocarpon*. Anecdotal reports suggest that *Rhizocarpon* section *Alpicola* appears late in a succession and is not necessarily found in *Rhizocarpon* communities of all ages in all areas (Innes, 1983b). If, for example, an aggregate-species curve constrained only by young control points is extrapolated for use on an older deposit, the curve may not account for the faster growth and possible differences in the mortality/recruitment of *Alpicola* on the older feature. Since the tempo of lichen succession and recruitment may vary within and between regions, studies that are concerned with the apparent inaccuracy of blended (an unknown mix of look-a-like species) "growth curves" should at least establish the surface age at which statistically significant differences exist in the sizes of *Rhizocarpon* section *Alpicola* thalli and those in the *Rhizocarpon* section *Rhizocarpon*. Innes (1983b) argued that this is sufficient reason to revisit and revise many of the European "growth curves". North American workers have been silent on this issue.

Some confusion might be eliminated if geoscientists were proficient at lichen identification. Unfortunately reliable identification of the yellow/green and black *Rhizocarpons* to a species level is an art/science that is best gained by hands-on tutelage by a well practiced lichenologist. These skills are not routinely taught in most North American universities. Indeed, the inspection of lichen spores and chemical testing cannot practically be done for every thallus. Some thalli are infertile, others are too small and immature to allow definitive identification and in some cases the various identification keys (e.g., Runemark, 1956) are not well suited for use outside of the geographic region in which they were developed.

Experiments

Reproducibility of sampling

Innes (1985, p. 231), notes that "individuals searching the same section of substrate may generate different data", that "two observers with the same search efficiency may generate different data depending on which thalli they locate", and that "it may not be possible for an individual to replicate the results obtained by another, whatever the sample area".

In our own experiment five individuals independently measured *Rhizocarpon* lichens on the bouldery rockfall or morainal deposit damming Moraine Lake in Banff National Park, Alberta, Canada (Wilcox, 1930). Three searchers had previous experience in lichenometry and two were neophytes. After a training session, each individual spent 3 h traversing the 160-m-long, 100-m-wide boulder deposit, trying to identify the largest thalli.

The searchers used the same methods but obtained widely varying results (Table 1). Surprisingly, no single measurement was repeated by a second researcher. This lack of consistency may in part be due to the time limit which may have been insufficient to allow a thorough search of the entire landform. However, since time spent sampling is not described in lichenometry papers, there is no reason to assume that all searches are thorough and complete.

Table 1
Largest *Rhizocarpon* thalli (long and short axes) found by 5 individuals on the Moraine Lake dam in Banff National Park.

10 largest lichens (mm)	R	J	A	K	D
1	190 × 140	250 × 227	167 × 153	201 × 172	230 × 220
2	220 × 130	230 × 210	212 × 152	186 × 166	170 × 160
3	145 × 119	213 × 196	230 × 150	150 × 132	150 × 130
4	180 × 118	214 × 145	152 × 135	203 × 131	150 × 120
5	145 × 115	196 × 187	142 × 125	170 × 130	142 × 120
6	140 × 100	198 × 168	145 × 117	170 × 119	190 × 110
7	91 × 100	190 × 160	153 × 104	153 × 119	130 × 100
8	210 × 95	185 × 161	142 × 98	191 × 118	100 × 95
9	145 × 95	180 × 118	127 × 87	123 × 114	95 × 95
10	116 × 94	124 × 118	125 × 86	198 × 113	160 × 90
Mean (large × short axis)	158 × 111	198 × 169	160 × 121	175 × 131	152 × 124

In addition to differences in observed largest thalli, there were differences in perception of what constitutes an acceptable thallus. One example was a large "lichen" that appeared as a scar-like patch (Fig. 3). Three searchers (including one with prior experience in lichenometry) said they would and two said they would not measure this scar-like thallus and recognize it as a weathered individual. Since this would have been one of the larger lichens found (250 × 160 mm, not included in any data set in the table) its inclusion would affect results, especially if larger thalli had not been found. While many would argue that the "lichen" in Figure 3 is clearly not a single thallus, the experience and sensibilities of individual members of a measuring party generally are not described in lichenometry papers. Our five workers also examined a thallus that four of the workers thought might have resulted from coalescence. While that thallus did not have a smooth thallus margin and a linear strip of hypothallus where areoles are absent, one of our test subjects was willing to accept it as an individual.

These examples call attention to the lack of formalized standards for search, acceptance and measurement. The results suggest that the reproducibility of lichenometric dating is questionable even if lichenometry is done by people who are similarly trained and are working at the same site.

Tests on substrates of known minimum age

Attempts to independently test lichenometry on surfaces of known age have been inconclusive and/or have shown that lichenometric ages did not closely estimate surface age. For example, Kirkbride and Dugmore (2001) measured lichens on Icelandic moraines that are independently dated with tephras. Moraines dated to the early to mid-18th century with tephra had lichenometric ages from the late-19th century, that is, the actual age of the moraines is roughly 100% greater than was indicated by the lichens. It is possible that the lichen community was disrupted by volcanism.

Matthews and Trenbirth (2011) directly measured for 25 yr the growth rate of the largest *Rhizocarpon* thallus (465 mm) on a boulder in a glacier forefield in Norway. Application of the growth rate to the 465-mm lichen indicates that it is about 1000 yr old. But the authors conclude from radiocarbon dating of soils that the boulder has probably remained undisturbed as lichen habitat for at least 8 ka.

In North America, the Recess Peak advance in the Sierra Nevada was long thought to be 2–3 ka in age, based on lichen sizes measured on moraines and an independently calibrated growth curve (Curry, 1969). However, these moraines are now known to be ca. 13 ka in age based on lake-sediment studies and cosmogenic analyses (Bowerman and Clark, 2011; Clark and Gillespie, 1997). Scuderi and Fawcett (2013)

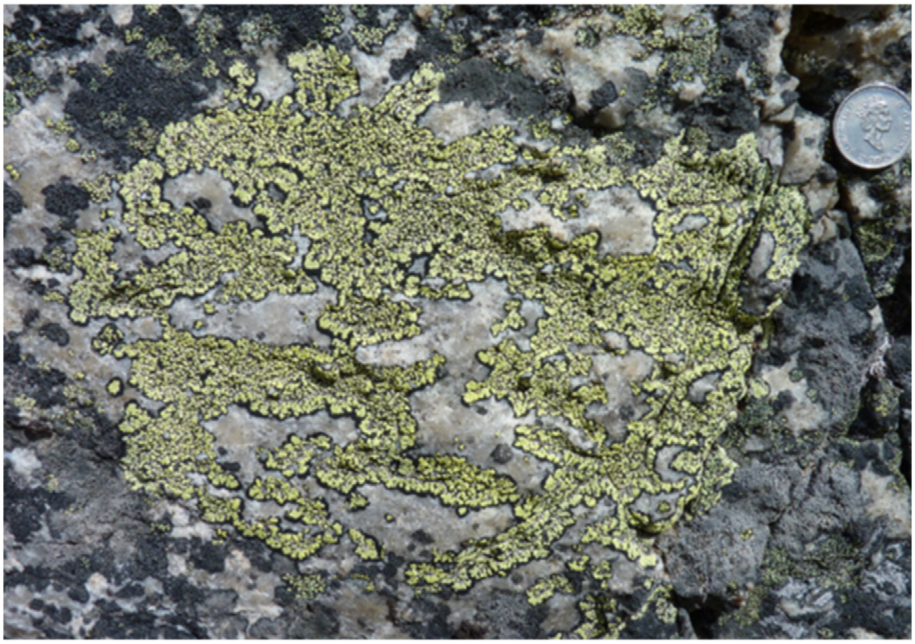

Figure 3. Apparent lichen scar with irregular *Rhizocarpon* thalli within. In an experiment most but not all lichen-counters considered this a non-usable specimen.

found no fault with the growth curve but concluded that original lichens had been killed off by a mid-Holocene advance of forests.

We did our own experiments on seven bouldery, minimally vegetated, lichen-bearing deposits in alpine environments, of the type usually dated by lichenometry. This included two sites in Banff National Park, Canada (Moraine Lake dam and the Crowfoot fossil rock glacier), two cirque moraines in Glacier National Park, U.S.A. (Sperry Glacier and north flank of Triple Divide Peak), two cirque moraines in Mt. Rainier National Park, Washington, U.S.A. (Hidden Lake moraine and Berkeley Park west moraine) and the moraine of a niche glacier above Fried Egg Lake in the Coast Mountains of British Columbia, Canada. On most of the boulders, yellow-green-black *Rhizocarpon* sp. thalli did not have marginal contacts and were not directly interfering with each other's growth. Our lichenometric age (Table 2) was estimated using the slowest published growth rate from any place in the North American Cordillera, which as far as we know is the Sierra Nevada curve of Curry (1969). These lichenometric ages are compared with known minimum or cosmogenic ages in Table 2. The spread between lichen ages and independent ages is actually greater than the table suggests: (1) Judging from current knowledge of regional late-glacial and Holocene glacier histories (e.g., Menounos et al., 2009), all moraines in the group are likely to be as old as the Fried Egg Lake moraine; (2) If lichen growth curves from the Canadian Rockies, Mt. Rainier, and the Coast Mountains were used at the respective sites (which is the normal practice), instead of the Sierra Nevada curve, lichen ages would be considerably younger.

These results, along with previous experiments, suggest that lichenometry cannot be relied on to provide moderately realistic numerical ages or even respectably close minimum ages. If growth curves (size-age scatterplots) can be taken at face value, it appears that the yellow-green-black *Rhizocarpons* grow for a millennium or two in some cases, but for only centuries in other cases. This further demonstrates that in many cases the fundamental assumption, that the ages of the largest lichens approximate the age of the substrate, is unwarranted.

Table 2

Comparison of ages for 7 alpine deposits in the Canadian Cordillera and Pacific Northwest. Lichenometric ages are calculated using the Sierra Nevada curve of Curry (1969) rather than local growth curves. Known minimum ages are derived from overlying tephra. Based on systematic mapping of alpine moraines in the Cordillera, actual ages of the deposits are likely the same as that of the Fried Egg Lake moraine, except for the Moraine Lake dam, which must be still older.

Deposit	Latitude/longitude	Substrate	Search details	Largest *Rhizocarpon* thallus (short axis mm)	Lichenometric age based on Curry (1969)	Minimum age (ka)
Moraine Lake dam	51° 19′ 40″ N 116°10′49″ W	Argillite	5 people, 3 h, entire deposit searched	250	6020	7700[a]
Crowfoot rock glacier	51° 38′ 27″ N 116° 24′ 50″ W	Mix of carbonates, quartzite, sandstone	2 people, 2 h, entire deposit searched	105	2530	7700[a]
Sperry Glacier moraine	48° 38′ 07″ N 113° 46′ 25″ W	Argillite	1 person, 2 h, entire deposit searched	59	1420	7700[a]
Triple Divide Peak moraine	48° 34′ 40″ N 113° 31′ 14″ W	Argillite	2 people, 1 h, entire deposit searched	35	840	7700[a]
Hidden Lake moraine	46° 56′ 28″ N 121° 35′ 56″ W	Andesite	2 people, 1.5 h, entire deposit searched	75	1810	2200[b]
Berkeley Park west moraine	46° 54′ 57″ N 121° 41′ 31″ W	Andesite	2 people, 1.5 h, entire deposit searched	68	1640	2200[b]
Fried Egg Lake moraine	50° 10′ 58″ N 122° 23′ 40″ W	Granite	1 person, 1 h, every boulder inspected	195	4700	11,000[c]

[a] Mazama tephra, ca. 6800 ^{14}C yr BP or 7700 ka (Bacon and Lanphere, 2006).
[b] Rainier R tephra, 2200 ^{14}C yr BP (Mullineaux, 1974).
[c] ^{10}Be ages average ca. 11,000 yr (Menounos et al., 2014).

It could be argued that only deposits that are obviously young should be addressed with lichenometry. But this is a circular argument; there is often no way to objectively assess the "youthfulness" of a deposit. For example, the Hidden Lake moraine at Mt. Rainier is the closest moraine to the cirque headwall, and bears trees no larger than those on some moraines in the Canadian Rockies that are taken on stratigraphic and morphological grounds to be LIA in age. The Triple Divide Peak cirque moraine is mostly barren of vegetation and apparently uneroded. Without the overlying Mazama tephra it could easily be taken to be a few centuries rather than several millennia old. Lichenometry would reinforce that misconception.

Is there any evidence that lichenometry works?

We have identified some invalid lichenometric dates and have explained why lichenometric dates in general cannot be trusted. Now we examine possible or at least claimed success stories.

It might be assumed by some that a linear alignment of points on a growth curve lends credibility to lichenometric dating controls. Yet McKinzey et al. (2004) challenged the accuracy of the historically calibrated straight line growth "curve" of Evans et al. (1999) in Iceland by comparing it with their own historically calibrated curvilinear growth curve. Points of both curves "lined up", but produced different results. Meanwhile, modeling experiments by Loso and Doak (2006) showed that the shapes and slopes of growth curves are greatly affected by lichen mortality, such that curves may have little connection to actual growth rates. Manipulations that "normalize" samples and subsamples so they "fit" lines that are described by long equations and statistical confidence intervals are suitably impressive, but the lichenometric ages may be precisely inaccurate. Even proving that the same results can be repeated does nothing to establish that ages are accurate, or if they are, will be accurate on anything other than the control sites.

Two studies in Alaska claim correspondence between lichenometric ages and ^{10}Be ages. The first is that of Young et al. (2009), who derived a 3 ka age for a moraine in the Alaska Range using ^{10}Be dating and the Solomina and Calkin (2003) revision of the *R. geographicum* growth curve developed by Calkin and Ellis (1980, 1984). Calkin and Ellis's (1980) extension of their growth curve out thousands of years beyond the oldest control point was not based on local data but "inferred from measurements of *R. geographicum* obtained in other areas of the world that are controlled by radiocarbon dates ranging from 1050 to 9000 yr BP" (p. 257). The Solomina and Calkin (2003) lichen growth curve was calibrated ca. 300 km south of this study site. It reports radiocarbon years converted to calendar years by the Calib rev. 4.2 program (Stuiver and Reimer, 1993).

A close reading of the arguments shows the apparent correspondences between lichen and ^{10}Be ages are not robust. The only control points over a few hundred years old for Calkin and Ellis's (1980) growth curve are ^{14}C ages of ca. 800 and 1300 yr BP on peat buried by 0.95 and 0.85 m, respectively, of sand and gravel on an alluvial fan. The related lichen sizes are from boulders on the surface of the fan. The buried-peat ages are minima, by unknown amounts of time, for the fan surface. Indeed, Calkin and Ellis (1984, p. 239) concede that the lichen sizes "must represent minimum figures. This is because of the time factor for deposition, erosion, or non-deposition which may have intervened between burial of peat and lichen colonization on the overlying substrates". This accurate assessment has not prevented treatment of the ages, and the resultant curve, as absolute by all subsequent users of the curve, including the authors listed above. In addition, the error spread in radiocarbon ages is dismissed: Solomina and Calkin (2003), when considering how to convert radiocarbon ages of the control points to calendar ages, note that "The 2-sigma interval is generally too large for lichenometric purposes, as some ^{14}C control point ages yield options up to a 600-yr range" (p. 136). They apparently fail to recognize the significance of those ranges to the utility of their growth curve. Finally, Young et al. (2009) apparently transcribe the growth curve erroneously for use in the Alaska Range. In the original Calkin and Ellis paper, and in Solomina and Calkin (2003), one thousand years is represented by 40-mm-diameter lichens. But in Young et al. (2009 Fig. 6 upper) the curve at 1000-yr exposure intercepts 75.9 mm and the lower diagram shows 1000 yr at 76.4 mm. We conclude that the correspondence of ^{10}Be ages and lichenometric ages in this case is a stroke of lucky coincidence, possibly aided by error.

The second Alaskan study is that of Badding et al. (2013), who compared ^{10}Be ages with lichenometric ages based on the *Rhizocarpon* curve of Ellis and Calkin (1984). They claim a correspondence between a ^{10}Be age of 4.6 ka and a 5 ka lichen age of a moraine in the Brooks Range. But the actual ^{10}Be ages on the moraine boulders range from 2.6 to 5.1 ka, and, as the authors note, there are a variety of possibilities for the actual age of the moraine. The lichen growth curve, as noted above, is extrapolated for ca. 3500 yr beyond the oldest dating control, which is the deeply buried peat. It is conceivable that lichenometry actually works in this case, but given the makeshift nature of the growth curve, the outcome seems rather serendipitous.

Yi et al. (2007) claimed that ^{14}C dating of the "largest dead lichen" on an outermost Holocene moraine in the Urumqu River valley, Tianshan, produced the same age as did lichenometry, citing Yi et al. (2004). This claim is apparently an attempt to bolster the validity of the lichen ages. But lichen thalli cannot meaningfully be radiocarbon dated because of carbon cycling (Garnett and Bradwell, 2010), and the authors are illogically comparing time since death of a thallus to elapsed time between lichen colonization and maturity. In any event, the original reference (Yi et al., 2004) makes no mention of dating a large dead lichen, claiming instead that lichenometric ages of a moraine match ^{14}C ages of carbonate crusts on a boulder and on a roche moutonee surface.

A few papers note similarity of lichenometric ages and tree-ring ages on young deposits (e.g., Harrison et al., 2007). But as noted before tree-ring ages are not necessarily any more robust than lichenometric ages. In the case of the Harrison et al. (2007) circularity is employed in one area: the ecesis interval for tree colonization is estimated using a lichen date.

Perhaps the most convincing examples of lichenometric validity are applications of the FALL (fixed-area largest-lichen) approach to multi-aged talus as demonstrated by Bull and coworkers (e.g., Bull, 1996; Bull and Brandon, 1998). Bull (1996) described how the FALL approach could be used to assign very accurate ages to paleoseismic rockfall events along the San Andreas Fault. He measured the longest axis of the largest lichen on each of hundreds of similar sampling units (boulders). Each boulder was presumed to be part of a single rockfall event, the fixed search area size was not specified, and the lichens were assumed to have always grown under similar conditions. In Bull (1996) the long-term lichen growth rate was estimated by measuring FALL sizes on two tree-ring dated landslides and on three historically dated sites. This tree-ring and historical evidence is the sole basis for "calibrating" the long-term lichen size-surface age relationship. Regression analysis was then used to fit a line to the lichen size-surface age controls for each of four lichen genera. Ultimately, the FALL thallus size data were transformed into a probability distribution where the peak in the Gaussian distribution of largest lichen sizes was matched with the lichen growth trend line to determine the age of the coseismic rockfall. Bull also suggested that differences in the amplitude of the Gaussian distribution that marks each rockfall event signify larger and smaller rockfall events. Overlapping distributions on the lichen size-surface age timeline were used to infer when prehistoric coseismic rockfalls happened.

The close match of the FALL distributions and the historical record of seismic activity is very impressive, but we question the objectivity, reproducibility and assumptions used in the FALL approach. We note for example that regression analysis was used to define the long-term lichen growth trend. This practice is only valid if there is normality of error, independence of error, and equal variance in all measured variables. Since no data are offered to show that unknown and unmeasured errors are normally distributed, use of regression in the FALL approach is not defensible —unless it "works." There is also reason to ask if the FALL approach is reproducible and "works" in any situation. If "incorrect" subjective decisions can lead to "incorrect" ages, failed tests of the FALL method could conveniently be attributed to user ineptitude rather than to the method itself. However, failed FALL tests and inaccurate ages could also arise due to a failure of the biologically unvalidated assumptions used in the FALL technique. Whenever FALL is used on surfaces of unknown age, we do not know if the key assumptions hold true for all lichen species, surfaces, timespans and environments. Unfortunately the technique is not sufficiently transparent to reveal if a user made "incorrect" choices at some sites and not at others.

The accuracy of FALL ages depends in large part on the assumption that there is a good fit between a probability function and lichen vital rates (i.e., lichen natality, mortality, growth). It also assumes that these vital rates are now and have been uniform across the study area. For example, Bull and Brandon (1998, p. 74) generated exacting "estimates" of lichen ecesis that brought their age estimates ever closer to the known year at which the rockfall occurred. This was done by treating lichen colonization as a Poisson problem and assuming that the colonization rate was uniform on all deposits. This approach seemed to work in this instance. However, Asta and Letrouit-Galinou (1995) and Clayden (1998) describe a much messier situation where spores are tiny and rocks have texture, pits, nubbins and cracks. Unlike fantasy spores that are sprinkled by a Poisson function, real spores are microscopic and are moved by wind and water. Bull and Brandon (1998) treated this as a Poisson problem and through their statistical manipulations they brought age estimates closer to their desired result. Perhaps this apparent success is an indication that colonization and mortality resemble Poisson functions. Alternatively, it might be argued that subjectivity played an unknown but important role. It is not reassuring to see that the FALL statistical manipulations do not well elucidate the magnitude of specific errors arising from user observations, unknown variability in lichen vital rates, and errors due to chance and biological unknowns. Lacking this information and more data about the uniformity or non-uniformity of lichen vital rates, we suggest that little trust should be placed in the FALL or any other lichenometric technique that treats biological processes as a Poisson problem.

Conclusions

The first clue that lichenometric results may be less than robust is the 50 years of disagreement over what to measure. Lichenometry has no standards and no definitive useful temporal range, the measurement of any size property of a lichen is problematic, and every search strategy ever proposed has been criticized for logical reasons. Radiocarbon and cosmogenic dating, in contrast, evolved after some trial and error into systems with generally accepted protocols and some substantive notion of possible error. No such history illuminates lichenometry. Methods, instead of settling down to standard and logically assembled protocols, proliferate more every year. The hundreds of individual approaches suggest that lichenometry may be an art, employing whimsical assumptions about organisms that we know very little about. It only *appears* to be a science. Indeed, questionable assumptions are accompanied by impressive quantitative wizardry, such as a variety of quantitative error ranges, and sophisticated statistical techniques that supposedly generate highly accurate age estimates. But these claims of high accuracy have not been validated with independent data. Bull's work is a possible exception but it does not involve dating single-age deposits. It would seem that subjectivity, opinion, and accuracy that is only nominal are acceptable so long as the resulting lichenometric ages seem reasonable.

Plastic rulers may have been replaced by calipers, but many of the same baseless assumptions that were long ago criticized by Jochimsen (1973) have been passed along from generation to generation. The most significant is the assumption that the largest lichen or lichens present on a rock surface colonized soon after exposure of the surface. Studies of lichen mortality put this notion to rest. Certainly on some young surfaces, and even rare older ones, the largest lichens may be first-generation old-timers. But on many older surfaces the original thalli may be long, long gone. There is no way to tell which is the case. Another case of wishful thinking, linear extrapolation of lichen growth curves, is just as dubious now as it was in Jochimsen's day.

If a few lichen dates turn out to match independently derived ages from the same deposit(s), it will suggest that lichenometry works in a few cases. But because it cannot be known ahead of time which measurements will lead to good ages and which to bad ages, the logical conclusion is that no assumption of good ages can be made. In the steps of the scientific method that most of us learned in our first science class, a proposed hypothesis is tested by means of experiment, and a conclusion is then reached. Plenty of experiments have shown that lichenometric dates are unreliable, and that becomes the conclusion.

We do not suggest that lichenometry is a totally useless exercise. Crustose lichens are often useful as relative-age discriminators, and can sometimes be used for example to distinguish adjacent alpine moraines of different ages. Large *Rhizocarpon* thalli indicate that a deposit is older than the Little Ice Age. Thallus sizes applied to a growth curve probably provide minimum ages of a deposit, because in most cases mortality effects may outweigh errors in application of the curve. But we suggest that the practice of assigning actual numerical ages to a moraine or landslide deposit on the basis of sizes of lichens is folly (folly in which we ourselves have engaged in the past), and persists not because it actually advances Quaternary science, but because it appears to be so easy and inexpensive. It may be that more rigorous approaches in the future may raise the reliability level, but presently, lacking data from controlled experimentation, meticulous longterm inventories and exacting measurements of lichen vital rates (e.g., recruitment, mortality, coalescence, and growth at different ages in different species in different environments) we run the risk of spinning our lichenometric wheels indefinitely. At the present time it is unclear if lichenometric reproducibility or the lack thereof occurs by chance, reflects changes in community dynamics, and validates or invalidates one or another methodology. Reliance on wrong assumptions, unvalidated measurements, quirky sampling designs and pseudoreplication will not get us out of this mess.

Hurlbert (1984), in a discussion of ecological field experiments, noted that "Poorly designed or incorrectly analyzed experimental work literally is flooding the ecological literature" (p. 207). We follow his cue and suggest that lichenometric ages based on wishful thinking, rather than verified assumptions, widely accepted methods, and reproducible data sets, are, if not flooding the Quaternary science literature, at least rendering it very muddy. Hurlbert concluded that the buck stops with journal editors: "When the coin of the realm is the published paper, nothing educates so well as an editorial rejection or request for major revision." (p. 207). Assuming that lichenometric believers continue to favorably review each other's manuscripts, it is unlikely that the level of rigor will improve until editors force it to.

A way forward?

It is not clear at this time whether lichen measurements will ever be able to provide reasonable numerical ages of geological substrates. But if the answer to that question is to be pursued scientifically, the first step should be abandonment of unverified assumptions. We make the following suggestions:

Modeling: The development and use of cohort life tables would offer assumption-free insights into demographic processes (Loso et al., 2014) and age-related changes in individuals and communities (Hestmark et al., 2004). Observation of thallus establishment and long-term tracking of those thalli can be done using repeated photography and measurements on orthorectified images.

Analytical accuracy and precision: Lichen measurements and identifications must be validated and reported (e.g., in an appendix). Herbarium specimens could serve as certified reference material (CRM) and the results of double-blind tests could be reported as proof that all members of the research team can reliably identify specimens and reject look-alikes and oddly shaped specimens. Measurement accuracy and precision should be fully revealed by using repeated measures and graphs (e.g., McCarthy and Henry, 2012, Fig. 4). Orthorectification must be a prerequisite for photogrammetry.

Hypothesis testing: Incremental advances can be realized by systematically addressing and testing carefully constructed hypotheses. Is the oldest thallus usually the largest? Is diametric growth even everywhere on a thallus margin? Is diametric growth closely correlated with age or areal growth? Do growth rates vary with rock-face aspect?

Legacy and novelty: Historically dated and permanently marked bedrock chronosequences should be established at accessible and protected sites. Detailed and shared datasets (at marked sites) should be published for use as reference populations against which newly proposed techniques could be calibrated.

Role of editors and reviewers: Data analysis and interpretation should be consistent with both the accuracy and precision demonstrated by the authors. The use of detailed appendices which will help workers independently replicate study results should be encouraged.

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