Size Effect in Penetration of Sea Ice Plate with Part-Through Cracks. I: Theory. II: Results[a]

Discussion by J. P. Dempsey[3]

A thorough examination of the quasi-static penetration of a floating elastic-brittle plate via a fracture mechanics approach has been presented by Bažant and Kim. Bažant and Kim reach the conclusion that there is a size effect (in terms of the plate thickness, h). A few of the assumptions made by these authors will be examined in this discussion.

The formulation presented by Bažant and Kim assumes both that a radial system of part-through cracks is formed and that the appearance of these radial cracks is accompanied by stable crack growth. The analysis proceeds by subdividing each part-through crack into narrow vertical strips (the ith strip being of length b_i, with ligament $h - b_i$). In each strip, the crack is assumed to propagate vertically, independently of the crack propagation in the adjacent strips. A simplified form of a cohesive crack model is adopted, with the crack initially growing as a plastic crack.

The assumed stable development of the part-through radial cracks does not match experimental observations, especially for thin to moderately thick ice sheets ($h < 0.5$ m). The initiation of cracks in ice almost always leads to unstable crack growth (DeFranco and Dempsey 1994). The radial cracking that occurs prior to the formation of circumferential cracks and subsequent penetration is understood to occur suddenly and to be through-the-thickness. In other words, a system of through-the-thickness radial cracks occurs, with rapid radial and through-the-thickness crack propagation. Even though these radial cracks are subjected to the dome or arching effect, crack growth instability in ice is sufficient to allow through-the-thickness cracks to form (in thick ice sheets, it is plausible to assume that the through-the-thickness cracking would be prevented by the arching effect). Dempsey et al. (1995) studied radial cracking with closure for the case of a clamped plate subjected to a concentrated lateral load. By assuming that the closure width was a function of the radial crack length only, Dempsey et al. (1995) obtained an analytical solution that facilitated a thorough examination of the dependencies of the closure width, the nucleated radial crack lengths, the energy release rate, and the penetration load. In particular, the latter analysis made it clear that radial crack growth instability would accompany the nucleation of any radial crack system. A finite-element study of a radially cracked floating plate by Sodhi (1996) confirmed the broad applicability of the conclusions reached by Dempsey et al. (1995).

An implicit requirement underlying the size effect analysis presented by Bažant and Kim is the stable formation of process zones (contiguous to each traction-free crack front) that scale self-similarly with the ice sheet thickness. However, if sudden and unstable radial crack formation takes place, with full through-the-thickness crack-face separation and subsequent compressive closure (unilateral contact, in other words), there is no logical way in which one can simultaneously assume the stable formation of process zones; there are, in fact, no ligaments subjected to bending, but instead pairs of completely separated crack faces subjected to ever-increasing pressure due to the arching action. This pressure grows to be of such magnitude that zones of circumferential microcracking in the plane of the ice sheet have been observed to occur, at variable radial distances away from the load. The radial crack lines have been observed to ‘‘whiten’’ with intense microcracking (Frankenstein 1966), and this is consistent with unilateral contact conditions of the receding type (Dundurs 1995), in which the extent of contact remains invariable with increasing load (in the case of elastic media; creep may alter this behavior, but not significantly). The issue of crack growth stability and whether the radial cracks would form stably or unstably was bypassed by Bažant and Kim, since they adopted the radial crack length a as the controlled variable. Their formulation, therefore, does not include a condition related to crack growth stability. By controlling the radial crack length numerically, their crack growth simulation is more stable than could be obtained in ice even under closed loop displacement controlled loading. For the majority of situations encountered, the much less stable condition of load control is operative.

For the case of relatively thick ice sheets, it is plausible that a radial crack system could form that would be comprised of part-through cracks. These part-through cracks would still form suddenly and, because of crack growth instability, would immediately partially close, with conditions of $K = 0$ along the crack front. Even on further loading, the remaining ligaments would be subjected to the compression induced by arching, and only during load-up would the crack fronts experience tension and process zone growth. The stable formation of crack-tip contiguous—but not necessarily self-similar—process zones would be expected to occur, but only for the case of rather thick ice sheets (thick here is estimated to mean $h \geq 1$ m).

If there is a size effect in ice thickness, it is important that it be determined, especially from the viewpoint of vehicles landing on, or traveling on, the ice. Safety is of primary concern in this case, and breakthrough is to be avoided. However, for the case of submarine surfacing, successful breakthrough is paramount, and a realistic load resistance estimate is all important. Given that the data in Fig. 5 of the authors' paper do not ‘‘visually demonstrate the invalidity of Sodhi's claim that there is no size effect,’’ one would intuitively favor a more conservative approach in the latter instance.

Conclusion: A fundamental requirement of a Bažant-type size effect analysis is the stable and self-similar growth of crack-front contiguous cohesive-type process zones. Such behavior is deemed implausible for the problem at hand. While a size effect may occur for thick ice sheets, it is unlikely to be significant for ice thicknesses less than 1 m.

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Discussion by Devinder S. Sodhi[4]

In their papers, the authors arrive at the conclusion there is a size effect on the failure load of floating ice sheets for ice thicknesses greater than 0.2 m. However, the results of their analysis are only useful if the assumptions made in their anal-

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ysis correspond to the real situation during vertical loading and breakthrough failure of floating ice sheets.

PART I

The process of a gradually increasing axisymmetric load on a floating ice sheet results in the following sequence of events: (1) elastic deformations; (2) formation of radial cracks; (3) wedging of radially cracked segments of ice sheets; (4) formation of many circumferential cracks; and (5) breakthrough due to large deformation or brittle failure of ice. If the loading rate is low, we also need to consider creep deformation of ice along with elastic deformation. During field tests, it is often difficult to observe their formation because of snow cover. During small-scale tests, the formation of radial cracks is a very short-time event. They propagate to a length of about 2–3 times the characteristic length and arrest. After the formation of radial cracks, compressive stresses in the top part of the ice sheet support the load because of the wedging or dome effect. The compressive stresses cause creep deformation of ice, resulting in further deformation.

The results of linear elastic fracture mechanics analysis are not immediately relevant to the propagation of cracks in a creeping material. The results of Slepyan (1990) and Bazant and Li (1994) are particularly flawed, because the interference between segments during elastic deflections of wedge-shaped beams was ignored. Dempsey et al. (1995) presented a formulation of plates having radial cracks with closure. Bazant et al. (1995) and Bazant and Kim (1998) consider closure of part-through cracks, and the failure criterion is the formation of the first circumferential crack. They did not consider the creep deformation of ice, nor did they consider the formation of multiple circumferential cracks, which have been observed in small-scale and full-scale tests. The authors arrive at a result that the dependence of breakthrough load P_f is proportional to $h^{3/2}$ using the results of field tests by Frankenstein (1963, 1968) and Lichtenberger et al. (1974). Those field tests were conducted by loading an ice sheet at a constant rate, and some of these tests lasted for hours. Therefore, it is not reasonable to use the results of those field tests to support the conjecture that fracture, while ignoring creep, gives the size effect $P_f \propto h^{3/2}$ for ice thickness greater than 0.2 m. Their criterion that an ice sheet fails when the first circumferential crack forms is also not correct, because many circumferential cracks form around the area of load application before final breakthrough takes place.

PART II

In their analysis, the authors considered a hole of radius equal to 10% of the characteristic length and assumed the load to be distributed at the periphery of the hole. Because there is considerable deformation of material in the area close to the center, the conclusion they have reached may not be totally correct.

On page 1320, they state that ''Frankenstein made extensive observations on lake ice, which can be assumed to behave similarly as sea ice.'' Yet they criticized Sodhi (1995b, 1998) at the bottom of page 1321 by saying that ''a second questionable aspect of Sodhi's (1995a,b) evaluation of test data is that he correlated in the same diagram the test results from different test series while implying the same ice properties. However, the ice properties were most likely quite different.'' Nevertheless, the authors plot the data from tests with freshwater and sea ice in Figs. 5(c and d).

On page 1321, the authors state: ''In view of the high scatter and limited size range of the available data, it cannot be claimed, however, that results actually prove the present theory.'' Yet the authors state on the bottom of the same page: ''Nevertheless, all the plots in Fig. 5 visually demonstrate the invalidity of Sodhi's claim that there is no size effect.'' In Figs. 5(a and b) of the paper, the authors have not really proven the existence of a size effect by fitting curves through three sets of data having high scatter and a narrow range of ice thickness.

In Fig. 6, results of small-scale and full-scale tests are plotted in terms of ice thickness versus failure load. This figure includes the data from ICEX-93 tests, in which ice penetration forces were measured during uplifting and breakthrough of floating ice sheets by two submarines (Dane 1993; Sodhi 1998). A line $P_f = 1{,}934\ h^2$ (where P_f is in kN and h is in m), obtained from the results of small-scale tests, passes through plots of full-scale data, which have considerable scatter. Because this line passes through the middle of the full-scale data, the discusser concluded that there is no size effect for ice thickness up to 2 m (Sodhi 1995b, 1998). Compilation of field data by Gold (1971) also supports failure load being proportional to the square of the ice thickness. Accepting the authors' conclusion that there is no size effect for ice thickness less than 0.2 m, the discusser has plotted a line representing $P_f \propto h^{3/2}$ in Fig. 6 from a point on the line ($P_f = 1{,}934\ h^2$), where ice thickness is equal to 0.2 m. This line does not fit the data obtained from full-scale tests on freshwater and sea ice.

The authors raise a point in the paper that the properties of

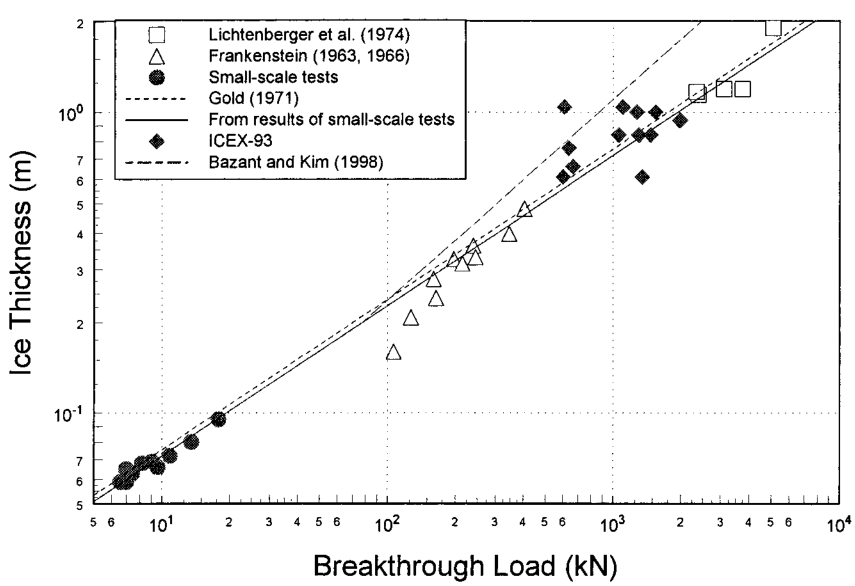

FIG. 6. Ice Thickness versus Breakthrough Load of Floating Ice Sheets

freshwater and sea ice may influence the failure load. However, the discusser considered creep properties of freshwater and saline ice and did not find much deviation between a line ($P_f \propto h^2$) and the estimated failure loads (Sodhi 1995a). The dependence of failure loads on salinity of ice appears to be a secondary effect, but its dependence on h^2 is supported by the strength failure criterion (Bazant 1993) because of creep deformation during wedging action.

On page 1322, the authors state: ''Sea ice exhibits creep, and the effective fracture energy as well as the strength depends on the rate of crack growth.'' Analysis of this problem incorporating creep will require abandoning LEFM, on which they base their present conclusions.

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Closure by Zdeněk P. Bažant,[5] Fellow, ASCE, and Jang Jay H. Kim[6]

DEMPSEY'S DISCUSSION

Dempsey's thoughtful and stimulating discussion is deeply appreciated by the writers. Citing certain simplifications made in the paper and revoking his own analytical solution, Dempsey states that dynamic fracture propagation instabilities may cause the size effect to be significant only for rather thick ice plates, thicker than about 1 m. Dempsey et al.'s (1995) elegant analytical solution, however, rested on even stronger simplifications, which render his conclusion about the lack of size effect for not too thick plates unjustified.

Dempsey assumes the cracks to reach through the full ice thickness, which implies the stress intensity factor K_I at the boundary of the crack closure zone (contact zone) is zero. Consequently, there is no dissipative mechanism at all in Dempsey et al.'s solution. No energy is dissipated by the fracture process as modeled. Despite the possibility of dynamic instabilities described by Dempsey, this seems to be a severe simplification.

Another drastic simplification in Dempsey et al.'s (1995) solution is that the depth profile of the open crack along the radial coordinate is assumed to be uniform from the load point up to the tip of the radial crack, with a discontinuous jump at the tip. The numerical solution in the paper, by contrast, revealed that the depth of the opened crack varies strongly with the radial coordinate and, at the radial crack front, approaches zero continuously.

The solution in the paper has proven that a static loading process cannot produce radial cracks that cut through the full ice thickness. Dempsey argues that full-through cracks are produced by dynamic instabilities, after which the crack partially closes because of arching (or dome) action. To support his view, he cites the fact that, in field experiments, the top surface of ice was seen to whiten along the radial cracks. This observation, however, does not proves Dempsey's point, in the writers' opinion. Cracks actually reaching the surface were not observed in the field. The observed whitening of the top surface of the ice was more likely caused by distributed cracking, which occurs in the fracture process zone of sea ice. The correct interpretation should be that the fracture process zone reaches close to the top surface. But this is not incompatible with the notion that the equivalent LEFM cracks reach to about 85% of ice thickness, as found in the paper.

Dempsey is not right in stating that ''the issue of crack growth stability . . . was bypassed by Bažant and Kim.'' Because, as shown in the paper, the vertical load increases with an increasing displacement, it is immediately clear that the solution obtained is stable (which means that this is a fracture problem of positive geometry, in fracture mechanics terminology). Contrary to Dempsey's comment, the solution is stable regardless of whether the radial crack length or the load-point displacement is controlled. The purpose of using in computations the crack length control instead of the displacement control was not to achieve stability of the actual response but merely to improve the convergence of iterations (or ensure stability of the numerical algorithm).

In principle, of course, it should not be ruled out that removal of some simplifying assumptions may lead to a significantly different solution exhibiting dynamic instabilities. There exist two possible sources of the dynamic instabilities emphasized by Dempsey: (1) strong inhomogeneity of sea ice; and (2) three-dimensionality of fracture propagation near the radial crack front, alluded to by Dempsey, which is undescribable by the assumed vertical propagation along an infinitesimal strip.

At the critical state of the stability limit, a structure is at the limit of static response (equilibrium). When stability is lost, the response becomes dynamic (i.e., there must be inertia forces to satisfy D'Alembert equations of dynamic equilibrium). Since the static solution for a homogeneous ice plate is stable, the only possible cause of unstable crack jumps (inevitably dynamic) is periodic inhomogeneity of ice properties. The value of fracture toughness K_c, considered constant in the paper, actually fluctuates randomly along the crack path (with some dominant wavelength l_c, representing the dominant spectral component of the random process of K_c as a function of crack path length).

In crack path segments in which K_c is decreasing fast enough, crack propagation may become unstable, dynamic. But it must be a snap-through instability, with a jump to a new stable equilibrium state, which must occur in the next crack path segment in which K_c is growing, constant, or not decreasing fast enough. Since every material is inhomogeneous, such instabilities occur in all fracture. They get manifested by acoustic emissions. Yet static LEFM still provides the correct approximation on the macroscale.

One might think that the rate of energy to form the fracture should be equal to the rate of stored energy release minus the rate of the energy radiated by acoustic waves. But the energy of acoustic emissions in ice may surely be considered negligible compared with the total energy needed to form the cracks. In concrete, for example, the acoustic emissions, due to snap-throughs at each fluctuation of fracture toughness caused by aggregate pieces, are as strong as in ice, yet it is generally accepted that the energy they radiate is insignificant compared with the energy required for concrete fracture. Otherwise, static fracture analysis of concrete would be impossible. Besides, it would actually be incorrect to subtract the energy of acoustic emissions, because it is never subtracted during the measurement of fracture energy. So the fracture energy value used in fracture calculations already includes the energy of acoustic emissions.

Dempsey apparently believes that the typical length of the segments of decreasing K_c along the crack's path (or the dominant spectral wavelength l_c, or the length of crack front jumps) is not microscopic, negligibly short compared with the radial crack length, but relatively long. But unless this length were

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comparable to the entire radial crack length (i.e., unless almost the whole radial crack forms dynamically), a static fracture analysis must still provide at least an approximate overall description, correct in the energetic sense.

Static approximations to dynamic instability in the form of a snap-through from one equilibrium state (the initial uncracked state) to another equilibrium state (the full-through crack with partial closure) must generally satisfy Maxwell's condition of energy equivalence (whose classical example is the Maxwell line through the instability in the van der Waals pressure-volume diagram for the vapor-liquid phase transition). But even if a dynamic snap-through from an uncracked state to a full-through crack followed by a partial crack closure were the actual fracture mechanism, Dempsey et al.'s solution does not appear to be energy consistent.

The solution in the paper, on the other hand, is energy consistent. Unlike Dempsey et al.'s solution, it guarantees the rate of release of the stored strain energy and gravitational energy of sea water to be equal to the rate of energy needed to form the radial cracks in ice, corresponding to the given value of the fracture energy of ice. Thus, the condition of overall energy balance is satisfied.

In view of the foregoing considerations, as well as the fact that no solution with a dynamic instability has yet been presented, Dempsey's concern about the dynamic instabilities appears exaggerated. It is clear from the solution in the paper that, under the assumptions made, the load is continuously increasing with the crack length as well as with the load-point displacement. This guarantees continuous stability up to the moment of formation of the circumferential cracks, provided that the ice is treated as homogeneous.

The second suspected source of error, the three-dimensionality, is reflected in Dempsey et al.'s solution to a lesser degree than by the solution in the paper. Dempsey et al.'s assumption that the depth of open crack along the radial crack is uniform, with a sudden jump to zero at the radial crack front (a place where the dynamic crack jumps would have to take place), is a rather severe simplification of a plausible fracture shape. In the paper, the open crack depth is variable and at the radial crack front approaches zero without any discontinuity. The depth variation is found to be quite significant. Therefore, the deviation from the actual three-dimensional behavior is evidently greater for Dempsey et al.'s solution.

It is strange that, while questioning the existence of size effect except in very thick plates, Dempsey ignores the evidence given by Fig. 5 in Part II of the paper. That figure shows the results of three field tests, and each of them clearly shows, despite high scatter, that a strong size effect is present even for a size range beginning with 0.1 m.

In conclusion, the writers remain convinced that the simplifications made in the fracture and size effect analysis of the paper were not unreasonable and that the numerical solution presented, with all its approximations, ought to be more realistic than the analytical solution of Dempsey et al., ingenious and elegant though it may be. In particular, the writers do not agree with Dempsey that a static analysis leading to ''stable and self-similar growth'' would be implausible. Simplified though the analysis in the paper obviously is, it nevertheless appears to be a reasonable simplification.

SODHI'S DISCUSSION

Sodhi has made some interesting and thought-provoking points. However, his severe criticism is unconvincing and, in the writers' opinion, invalid.

It is true that the neglect of radial crack closures in Slepyan (1990) and Bažant and Li (1994) was an oversimplification, but these early studies, judged as ''particularly flawed'' by Sodhi, represented necessary steps in the evolution toward a realistic fracture analysis and clarified some important aspects of the scaling problem. Prior to Dempsey et al. (1995) and Bažant et al. (1995), no fracture studies of ice plate penetration took the crack closures with the inherent dome effect into account (some limit analysis studies did, but to treat ice as a plastic material without softening damage is a much more serious ''flaw,'' in the writers' opinion).

There is no dispute that certain simplifying assumptions were made in the paper, but the writers believe them to be reasonable and sufficiently realistic. One simplification was the neglect of creep, which is repeatedly reproached by Sodhi. However, assuming that creep would not mitigate the size effect is not baseless.

There used to be a widespread intuitive misconception that the influence of creep is like that of plasticity, which tends to increase the process zone size, thereby making the response less brittle and the size effect weaker. But the influences of creep and plasticity are very different.

The influence of creep on scaling of brittle failures of concrete, which is doubtless quite similar from the mechanics viewpoint (albeit different in physical origin), was studied in depth at Northwestern University, along with the effect of the crack propagation velocity; see, e.g., Bažant and Gettu (1992); Bažant et al. (1993); Bažant and Planas (1998); and especially Bažant and Li (1997) and Li and Bažant (1997). The conclusion from these studies, backed by extensive fracture testing of concrete and rock at very different rates, is that, unless creep actually prevents crack formation, creep in the material always makes the size effect due to cracks stronger. In the logarithmic size effect plot of nominal strength versus structure size, it causes a shift to the right, toward the LEFM asymptote.

In light of these studies, Sodhi's claim (in his last paragraph) that ''incorporating creep will require abandoning an LEFM approach'' must be seen as erroneous. The opposite is in fact true: The slower the loading (or the longer its duration), the closer to LEFM is the size effect in a cracked structure. The physical reason, clarified by numerical solutions of stress profiles with a rate-dependent cohesive crack model (Li and Bažant 1997), is that the highest stresses in the fracture process zone at the crack front get relaxed by creep, which tends to reduce the effective length of the fracture process zone. The shorter the process zone, the higher the brittleness of response is and the shorter the size effect. This explains why experiments on notched concrete specimens consistently show the size effect to be stronger at a slower loading (Bažant and Planas 1998). It is highly probable that the same will be verified for ice, once size effect tests at very different loading rates are carried out.

From the aforementioned studies, it thus transpires that, in order to take the influence of creep on the size effect approximately into account, one does not need to abandon equivalent LEFM, as claimed by Sodhi. It suffices, in the case of very slow loading, to reduce the value of fracture energy (or fracture toughness) and decrease the effective length c_f of the fracture process zone. Even these adjustments, however, are important only when loading durations differing by several orders of magnitude are considered, which is not the case for the ice penetration tests cited by Sodhi.

Sodhi also states that considering the load to be applied along the circumference of a hole of a radius of about 10% of the characteristic length must have caused the results not to be ''totally correct,'' apparently meaning not totally representative of the idealized case of a concentrated load applied at a point. However, the conclusions ought to be essentially correct. Fracture is at a maximum load driven by the global energy release from the ice plate—sea water system and is not very sensitive to local disturbances near the load application point, where reach is limited according to Saint-Venant principle.

Sodhi's comments in the second paragraph of Part II are taken out of context and result from a misunderstanding of the criticism in the original paper of Sodhi's previous way of handling the available data sets. In Figs. 5(c and d) of the paper, cited by Sodhi, the coordinates are not the actual thickness D and nominal strength σ_N but their relative values, which are normalized by the values of $\lambda_0 l_0$ and Bf_t only *after* these values have already been determined for each data set separately. The two plots were presented in the paper merely for visual demonstration; they were not used for actually identifying the material parameters from the test data. On the other hand, in his previous works cited from the paper, and again in his present discussion, Sodhi plots the data from different data sets in the same plot and actually uses regression in this plot to determine the parameter values. The criticism of such a procedure stated in detail in the paper is valid.

Since the relation of the ice properties in various data sets is not known a priori, an arbitrary vertical or horizontal shift (in log σ_N) of the group of data points from one data set against that from another data set is allowed and must be considered. Just by choosing a suitable vertical or horizontal shift of the data groups, any desired conclusion can thus be obtained—the presence of a strong size effect, or the absence of any size effect (in Sodhi's case). Nothing is thus proven by Sodhi's plot. This is the salient point criticized in the paper.

The kind of plot shown in Fig. 6 and discussed in Sodhi's fourth paragraph, Part II, is misleading for two reasons: (1) as known from Buckingham's theorem of dimensional analysis, general physical laws are correct only if they can be written in a dimensionless form; and (2) the breakthrough load P_{max} must obviously depend on ice strength f'_t. To achieve a dimensionless coordinate, the breakthrough load in Fig. 6 must be divided by $f'_t h^2$, h being the ice thickness (a division by f'_t amounts to a horizontal shift in the logarithmic scale). But then it is not a priori clear how the f'_t values for different data sets relate to each other, because they have not been separately identified in advance.

Consequently, the relative horizontal positions of the groups of circles, triangles, diamonds, and squares in Fig. 6 must be considered as undetermined in advance. This implies that Sodhi's plot in Fig. 6 can be valid only for one kind of ice, not for different kinds simultaneously. Arbitrary vertical shifts of one data group against another, due to unknown differences in f'_t, would have to be considered in Fig. 6 if the breakthrough load were normalized by the ice strength. [Here the shifts are not vertical, as considered in the paper, but rather horizontal, because Sodhi for some reason inverts the coordinates; the ice thickness (normalized) would normally be the coordinate and the breakthrough load (normalized) the ordinate.]

The ice thickness h in Fig. 6 should of course also be normalized to yield a dimensionless coordinate. One way to do that might be to adopt as the ordinate the dimensionless parameter $h\rho_w/f'_t$, where ρ_w is the specific weight of water (of dimension N/m^3). In that case, the vertical and horizontal shifts in Fig. 6 are the same and thus the plot looks the same after the shifts. But ρ_w/f'_t is not the only possible normalizing factor for h and is in fact not the most reasonable one.

If fracture plays any role, then either the characteristic length l_0 of the cohesive crack model or the effective length of the fracture process zone in the sense of equivalent LEFM must somehow appear in the solution. So the ice thickness h should correctly be normalized by l_0. In other words, the ordinate h in Fig. 6 should be replaced by the relative thickness h/l_0. With this reasonable normalization of h, the arbitrariness of the horizontal shifts pointed out in the previous paragraph remains. Ignoring this kind of normalization of h, which is implicit to Sodhi's approach, is tantamount to assuming a priori that fracture mechanics plays no role in the problem and that there is no size effect. Given that such a hypothesis is implied, Sodhi's use of Fig. 6 to dismiss the size effect appears to be a circular argument.

Still another noteworthy point, already made in the paper, is that the coordinate of the size effect plots should not be the load P but the nominal strength $\sigma_N = P/h^2$. The case of no size effect then corresponds to a horizontal line. The plot in terms of P superimposes on the size effect the underlying proportionality of P to h^2 corresponding to the strength theory, which does not represent a size effect as generally understood. This obscures the size effect, as demonstrated by Figs. 4(b and c) of the paper. Sodhi does not question this demonstration, yet he persists in his discussion in plotting the size effect again in terms of P rather than σ_N.

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