An open letter on the "generalised tipping curve" of the SIM pulling tests, the "dynamic" SIM wind load analysis and VTA

This paper is an open letter, as answers and explanations from the herein mentioned authors would be warmly welcomed

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Abstract
Current methods such as the Visual Tree Assessment (*VTA*) of Mattheck and the pulling tests of Wessolly (*SIM*) and others will be analysed here. A number of authors have asserted since 1998 that their "generalised tipping curve" (*GTC*) would predict the uprooting of trees by extrapolating small values of stembase tilt angle under a static pull, and that 100% of the maximum or critical uprooting moment (M_{crit}) would be reached at 2.5º. However, evidence elucidated from literature and pulling tests strongly suggest that a very different tipping curve (similar to a curve published in 1965 by a comparatively unknown researcher) has been used instead for the pulling tests, where 1º = 100% M_{crit}. That curve will be called the *SIM* curve herein. A number of related researchers report contradictory findings, by showing either uprooted trees that had obeyed the *GTC* (2.5º) or trees that had perfectly obeyed the *SIM* curve (1º) which is confusing. This paper also analyses highly-cited and influential proclamations regarding the *GTC* and the *"dynamic"* versus *"quasi-static" WLA,* that may have, quite simply, sprouted from unconscious research bias.

Introduction
The assessment of the risk of trees and palms falling down (uprooting or breaking) plays a pivotal role in the arboricultural industry. And this, not only because of legal and physical concerns, but also because a reliable method would be the "Holy Grail" and an extraordinary source of financial wealth.

Current leading methods for assessing the uprooting and breaking potential of trees and palms are based on simple beam theory (*BT*). The *SIM* pulling test method or "tree-statics" were published by [9-13] and others. The tree is envisaged as a beam that is firmly fixed and a static bending moment would cause axial stress to exceed the wood's strength. The curvature of the beam is assumed to be negligible and it is made of an isotropic material that does not damp any mechanical energies. The *SIM* pulling test method or "tree-statics" were published by [9-13] and others.

With the pulling test (*PT*), a hypothetical safety factor for breaking is obtained by measuring the microscopic longitudinal deformation of the peripheral fibres with strain-gauges or elastometers while the tree is statically pulled. Then, that strain is correlated according to Hooke's law (*ut tensio sic vis*) with the static pull load and a hypothetical "dynamic" (in earlier publications) or "quasi-static" (in recent publications) wind load analysis (*WLA*), to estimate primary failure of the fibres on

the compression side. Experiments with strain gauges on leaning branches have been conducted too assess their predictive value.
The uprooting potential for a design wind speed is also extrapolated with a "tipping curve" as the basal deflection is recorded with tilt sensors or inclinometers. The root system is assumed to be either a freely-rotating semi-spherical mass which self-weight would counteract a static bending moment or a deformable horizontal beam which would fulfill the same function (resembling an inverted "T" or a horizontal "S"). Then, the values are extrapolated according to the famous *"Generalised Tipping Curve"* (*GTC*), published from 1998 onwards. The values obtained would then predict the uprooting safety factor (in %) by comparing a hypothetical wind load with an alleged critical load for uprooting, the latter based on the *GTC*.
However, the *GTC* is invariably shown as a diagram without explaining the mathematical equation behind it.
According to those authors, 40% of the critical uprooting moment (M_{crit}) is reached at an inclination (θ) of 0.25º of the stembase, while 100 % of M_{crit} would be reached at θ =2.5º. The *GTC* is allegedly the result of uprooting 400 trees: *"The generalized tipping curve was derived from scientifically based destructive pulling tests of more than 400 trees of different species under different soil conditions"* [sic. 12]. Wessolly and Erb ([10], p. 278) refer to that curve as *"discovered by the author" (sic.)*, and victoriously proclaim (pp. 161-162): *"...nearly all trees bend in an almost identical arc...", "...no matter whether the roots are intact or damaged..."* and *"...the stability of a tree can be determined accurately..."* (*sic.*). And: *"Applies to all trees"* (*sic.* [10] diagram, p. 163). And on p. 242: *"Inclino method (SIM) pulling test...diagnostic accuracy...very good"* (*sic.* [10]). However, no clear references were found in [9-12] pointing towards scientific, peer-reviewed papers relating to scientifically sound data and scientifically contrasted procedures that would support their claim.

Other arboricultural companies have developed and marketed their own brand of pulling test in their wake. However, it is unknown to the corresponding author if those other arboricultural companies/brands have simply copied the original *GTC* into their own pulling tests or not.

The issue with such extrapolations is establishing a threshold or critical angle. For instance, [14] do not set that threshold, by which the predictive value of their method could be anywhere between 0 and 100%. Their motion sensors just indicate the tree's basal inclination under winds and establish a correlation for each individual tree. But without a threshold (e.g. 1º or 7º), their method says not much about the tree's stability and it remains thus guesswork. The following quote evidences this fact when they say *"...trees that tilt 0.25 degrees or more at 50 km/h will have an inclination of 0.6 degrees or more at high windspeeds..."* (sic. [14]). It is difficult to be more imprecise and obscure than this in a scientific journal. Nevertheless, these same authors unwillingly also helped to reveal the real and unpublished SIM curve as used in 2003-2004 as will be explained further. And related authors [15] also publish an entirely different uprooting curve.

Related researchers have victoriously proclaimed that their commercial *WLA* software packages would incorporate *"dynamics and natural frequencies"* (e.g. [9-12]). For instance, Detter et al. ([11]) stated: *"...(aerodynamic drag in tree crowns, enhancing effects due to oscillation) were incorporated in the original equations."* (*sic.* [11]). And Wessolly and Erb ([10], p. 234): *"...the load analysis of the crown, taking ... into consideration... and the tree's ability to oscillate".* And: *"Dynamic components of the wind and of the tree are also taken into account"* (*sic.* [10]).
But no evidence could be found to support their claim [4].

The wind load was thus assumed to be dynamic, while the same authors recently proclaimed that it would be "quasi-static". Highly-cited researchers in forestry support this mechanistic model, while the ones in urban environments market commercial static pulling tests.

Another popular method is to combine a *WLA* with *BT*, i.e. a static bending moment that would cause axial stress to exceed the wood's strength, and often combined with decay detecting devices such as drilling or tomography. An an in-depth analysis of commercial tools, research claims and an overview of their possibilities, limits and predictive value were given in [3-8] and is not repeated here to avoid redundancy.

And, on the other side of the spectrum, there is the Visual Tree Assessment method (*VTA*):

Several decades ago, two highly-influential ideas were published: the *"axiom of uniform stress"* and the *"compression fork"* model [16]. According to the axiom, a uniform stress level would be maintained by compensation wood. This led those authors to propose "outer symptoms" of inner mechanical defects (e.g. decay) like, for instance, swollen areas, wrinkles and bulges. The presence of those symptoms would then require further inspection with commercial decay detecting tools (e.g. micro-drilling, the "Shigo meter" or acoustic tomography). Or the Fractometer, also marketed by the author of [16] together with his books and workshops.

Meanwhile, another popular and die-hard idea in arboriculture is that branches with included bark would push each other outwards, leading to possible fracture, which was also published by the same authors of [16] and which was seemingly unsupported by scientific evidence. Logical reasoning was offered in [7] that shows that those two ideas, when combined, appear to be self-invalidating.

The *VTA* method also offers criteria for the acceptable hollowness of trees, with the detection of external symptoms of internal faults. The famous *VTA* rule was supposedly a critical threshold of the ratio of the residual wall thickness versus the radius of the cross-section (*t/R*). Hollow dicotyledon trees would present a higher risk of breakage if that ratio were lower than t/R=0.32 (or 70% of the cross-section damaged) [17]. And that rule, easy to grasp by arborists and practitioners, has been used world-wide for the breaking risk assessment of hollow tree and palm stems, while has been successfully marketed in combination with commercial decay detecting tools. Nevertheless this ratio was recently scientifically refuted in [3] while it has been attributed to falsification too (Grüber in [3]).

It will be shown that the published *GTC* is suggestive of being misleading or erroneous information, as other tipping curves were found. A literature review of publications related to *VTA* and *PT* suggests that related authors have published contradictory and even self-invalidating findings.

It will also be shown how publication bias and research bias may be responsible for the high prevalence of papers and researchers who support *VTA, BT* and *PT* and related commercial devices. It is also suggested that this high prevalence of seemingly unsubstantiated or contradictory claims, may suboptimally be channelling the course of funded research, while shaping the concept of "structural stability" as perceived nowadays in the arboricultural and forestry industry. Examples will be given to show how apparently obscure or flawed proclamations have survived or subtly been adapted since 1998, to finally appear as "scientific" in 2021, although their path seems to be strewn with discrepancies and incongruencies.

Some guidelines will be given to simulate *WLA* and uprooting predictions, coupled with motion sensors and critical wind speed calculations, on a simple spreadsheet.

From here onwards, the whole body of the Tree Structure (aerial parts and roots) will be denoted *TS*, as trees may fall down by structural failure of the roots too (e.g. snapping, tearing or splitting) and not only because of "uprooting" (i.e. a shallow turned-up root plate). This is important, as terms such as "uprooting" or "tipping" often imply forcing and channelling our minds towards the over-simplified models such as used in pulling tests or mechanistic forestry models.

Materials and Methods

Compliance with guidelines and legislation: Experimental research and field studies on plants (either cultivated or wild), including the collection of plant material, must comply with relevant institutional, national, and international guidelines and legislation. However, plants or trees were involved now and the present study only analyses previously obtained empirical data and published claims.

Uprooting

The corresponding author had always perceived the published *GTC* of [9-12] as "odd". Perfect values such as 0.25º and 2.5º are related to perfect outcomes (40% and 100% respectively) and can be found everywhere (e.g. [9-13]). And Detter and Rust ([13], Fig. 6) claim that M_{crit} equals 2.5 times the moment needed to reach an angle of 0.25º. All this looked geometrically too good. So, a simple five-minute experiment sufficed to reveal another curve than their published *GTC*:

First, 0.25 was multiplied by 2.5, giving 0.625. This value is also, coincidentally, the angle at which 100% of M_{crit} is reached in [15] who also support the pulling tests. And again, this looked geometrically too good. Then, 0.25 was divided by 2.5, giving 0.1. And as the only changing parameter is *θ,* and since the *GTC* looks exponential, it was just a matter of experimenting for a few minutes to find the following equation:

$$\%M_{crit} = \left(\frac{0.25}{\left(\frac{0.25}{2.5}\right)}\right)^{(0.25*2.5)}$$

Eq. 1

This equation gives 31.53% of M_{crit} when $\theta = 0.25º$, which is thus surprisingly close to 40%.

Next: Detter and Rust ([13], Fig. 6) say that the estimated allowable load would be 130 kNm and that that would agree exactly with an angle of 1º. Then they say that 0.25º equals 40% of the estimated allowable load (thus: 52 kNm x 2.5 = 130 kNm). So, the more precise parameters were found in the following manner:

$$Parameter\ 1 = \frac{0.25*100}{40}$$

Eq. 2

$$Parameter\ 2 = \frac{\sqrt{Parameter\ 1}}{10}$$

Eq. 3

Hence, Parameter 1= 0.6250 and Parameter 2= 0.0791. Then Parameter 1 had to be slightly adjusted manually, to agree better with $\theta = 1^o = 100\%\ M_{crit}$

$$\%M_{crit} = \left(\frac{0.25}{(0.0791)}\right)^{(0.6250+0,0207)}$$

Eq. 4

Surprisingly, Sagi et al (2019, [48]) also show a tipping curve where $\theta = 1^o = 100\%\ M_{crit}$.

The equation published in 1965 and borrowed by Sagi et al (2019, Eq. 12) from Brinch-Hansen (Brinch-Hansen (1965) in Sagi et al., 2019), gives 100% $M_{crit} = 1^o$ too:

$$M_o = \theta_o^n + \alpha . \theta_o (1 - \theta_o^n)$$

Eq. 5

Where:

Mo = the moment normalized by the failure value, in kNm;

θ_o^n = rotation normalized by the failure value (1º);

α = the soil constant (1);

n = the soil constant (0.64).

Sagi et al (2019) show a formula that had already been published in 1983 *("The rotational stiffness*

(Kθ) of a rigid surficial circular foundation can also be determined from classical elasticity theory (e.g. Gazetas, 1983)" sic. [48]).

$$K_\theta = \frac{8Gb^3}{3(1-\mu)} \qquad \text{Eq. 6}$$

Where:
K_θ = rotational stiffness, in kNm/degree;
G = the shear modulus (11 Mpa);
b = the radius of the root plate (0.52 m);
μ = Poisson's ratio (0.5).

The forest tree pulled over in Sagi et al (2019, [48]) clearly has an ideal and modelable "*..rigid surficial circular foundation.." (sic* [48]) and does not seem to be affected by decay or other defects as commonly found in urban environments.

And the hypothetical validity of Eq. 4 was also confirmed by the following finding:

In 2015 Göcke and Rust [18] stated: *"...the tilt values of 0,2°- 0,3° at 50-60 km/h automatically lead to critical tilt values at gale wind speeds (90 km/h..."* (sic. [18]). A tilt value of 0.25° at 50 km/h corresponds to the "critical tilt value" of 1° at 84,39 km/h. And 1° is reached at 88,69 km/h if 0.25° coincides with 60 km/h [18]. Hence, their correlation diagrams of *"Tilt record 1: Picea tree"* and *"Tilt record 2: Tilia tree (in summer)"* ([18], p. 2) *also* agree with Eq. 4 (Table 1).

From here onwards, Eq. 4 will be denoted as the "*SIM*" curve.

Table 1. Eq. 4 or the *SIM* uprooting curve agrees surprisingly well with the correlation diagrams published Göcke and Rust [18].

formula SIM			formula SIM		
50 km/h			60 km/h		
Recorded wind speed=	13,89	m/s	Recorded wind speed=	16,67	m/s
Recorded inclinometer=	0,20	degrees	Recorded inclinometer=	0,30	degrees
	35,10	% of Mcrit		45,78	% of Mcrit
Critical wind speed=	**23,44**	**m/s**	**Critical wind speed=**	**24,63**	**m/s**
	84,39	**km/h**		**88,68**	**km/h**
Tilt record 1, Picea tree			Tilt record 2, Tilia tree in summer		
56 km/h			54 km/h		
Recorded wind speed=	15,56	m/s	Recorded wind speed=	15,00	m/s
Recorded inclinometer=	0,25	degrees	Recorded inclinometer=	0,25	degrees
	40,63	% of Mcrit		40,63	% of Mcrit
Critical wind speed=	**24,41**	**m/s**	**Critical wind speed=**	**23,53**	**m/s**
	87,86	**km/h**		**84,72**	**km/h**

The following procedure also allowed to test the validity of Eq. 4:

Eq. 4 was compared with the results of the *SIM* pulling tests carried out by the corresponding author in 2003-2004, to see if there was any agreement too. A number of Inclinometer measurements (n = 234) and corresponding safety factors for uprooting ($SF_{Uprooting}$) were taken from 14 pulling tests. The raw, uncalculated data had been emailed to Brudi and Partner Tree Consult (authors of [11]) who ran the raw data through their *SIM* pulling test software, to return the final results to the corresponding author as pdf-files. The pdf files were purchased by the corresponding author for its posterior use in workshops and his arboricultural consultancy services.

The correlation between M_{crit} and θ was obtained by calculating backwards the known results (starting from $SF_{Uprooting}$) from the pulling tests:

Step 1: $(Mprop/ SF_{Uprooting}) = M_{crit}$
Step 2: a table is made with two columns, containing measured θ and calculated M_{crit} respectively;
Step 3: this data was run through a software [19] to find the correlation and the fitting equation

(similar to Eq. 2 but with entirely different parameters) that would enable prediction of M_{crit} by simply inputting θ.

The correlation found for the SIM software was confidentially shared with the editor and referees. There may be a slight variation between the equation used in the *SIM* software of [11] and the one found herein, as the original data on the pdf files had been rounded off: *i.e.* a safety factor for uprooting of e.g. 67% (on the original pdf sheet) might have been e.g. 67,49% (in the original software). And, as the rounded-off values were used herein, the new-found curve, calculated backwards from the rounded-off numbers, may slightly deviate, as it will give, again, 67% instead of 67,49%. But this does not affect the crucial observations made in this study.

The original pulling test sheets in pdf were shared confidentially with the peer-reviewers, to ensure full reproducibility by the latter and also to enable unbiased and independent confirmation of the mathematical precision of the corresponding author's findings. In this way, statements regarding the *"inclinomethod"* (e.g. [9-15,18]) could finally be tested.

The beam fixed on a half sphere

Several authors in forestry have proposed critical uprooting moment predictions, considering an inverted cone-shaped (e.g. [20]) or a half-spherical root-soil plate with a certain radius (*Rr*) and density (ρ, kg/m³) [21]. Gravitational force would then act as an effective lever to oppose a bending moment caused by wind in the crown. The stiff beam is firmly fixed to the sphere and the latter rotates according to the force at the tip of the beam, neglecting shear and friction between the "root-ball" and the surrounding soil. If the density remains the same (e.g. $\rho = 2500 kg/m^3$ from [15]), then the *sole* predictor of the critical moment is the radius of the root plate (Rr_{Min}), for any given tree and soil condition (e.g. dry, frozen or water-logged).

First, the critical moment M_{crit} is taken from both tipping curves: 0.25° = 40 and 1° or 2.5° = 100%). Then, the corresponding critical Rr_{Min} can be calculated by inverting the equation of [21] and taking $\rho = 2500 kg/m^3$ and $g = 9,81$ m/s:

$$Rr_{min} = \sqrt[4]{\frac{3*M_{crit}*1000}{g*2*\pi*\rho}}$$ Eq. 7

The third uprooting curve

The *FE* model of [15] was a beam fixed on rotating a half sphere. Their model was said to be validated, as the correlation sufficiently predicted moment versus inclination, for both the *FE* model and a real uprooted tree. The correlation for moment versus angle was described as (Fig. 5A in [15]):

$$M = 368.2*\theta*0.01*100$$ Eq. 8

Where:

M = the moment related to θ, in kNm.

Here, a *M* of 92.05 kNm is given at 0.25°, which would be 40% of M_{crit}. Then it follows that 100% M_{crit} = 230,15 kNm. Which now corresponds to θ = 0.625°.

Dynamics

The equation for brick and steel chimneys from [6] (p. 256, Eq. 11) was used here:

$$n=\frac{\left(\frac{\varepsilon*d}{h^2}\right)}{100}$$ Eq. 9

Where:

n	= the bending-frequency of the trunk, expressed (Herz);
ε	= the factor of frequency (4420);
d	= the diameter of the trunk (cm);
h	= the height of the tree (m).

The corresponding author found this equation coincidentally in 2005, when he was trying out a number of published (and mostly very complex) equations with the aim to discover the equation behind that highly-cited and much-celebrated claim (*"static pulling with dynamic wind load assessments"*).
The wind load analysis and pulling test files (an undisclosed number that was high enough to provide substantial verification) were shared with the peer-reviewers to ensure the correctness of this surprising finding. A wide range of specimens were included (palms, deciduous open-grown trees and conifers, all in urban environments). The raw data had been recorded by the corresponding author, then the calculations had been carried out by [11] in 2003-2004 and the pdf files purchased by the corresponding author for its posterior use in workshops and his arboricultural consultancy services. In this way, statements regarding pulling tests, *"dynamic wind load assessments"* or *"quasi-static"* (e.g. [9-12] could finally be tested.

Literature review

The review of publications related to the *GTC*, *WLA,* pulling tests and *VTA* was conducted as follows: Searching on Google (incognito mode) was chosen, as this enabled to detect highly-influential authors and claims in grey literature (e.g. articles in magazines, conference papers) and non-indexed predatory journals, that would not have been detected in databases that are commonly used for academic purposes (e.g. Scopus or Pubmed). Search terms evolved around the following: 'generalised uprooting curve', 'dynamic wind load assessment trees' and 'visual tree assessment + compression folds'. Then, the process was continued by manually selecting other publications that were cited in the publications retrieved during the first search. The cycle was repeated until sufficient evidence was found to support the analyses. And as a considerable number of articles was found with a rather poor and non-rigorous appearance and seemingly evolving around the same club of authors, the search was enriched by combining terms such as e.g. 'Forests', 'pulling tests' and 'resistograph'. Publications were sought for that offered commercial tools, alleged solutions for the risk assessment of trees or have been influential in the arboricultural and forestry fields and that were online available for free (Open Access). Some of the reviewed publications had already been retrieved for [3-8]. Unfortunately, some publications are available to members only and could thus not be scrutinised (e.g. *Arboric. Urban. For.* of the International Society of Arboriculture).

Limitations of this study

So as not to exceed manuscript length, only a small sample of examples could be included. Hence, the remarks and observations made may not be limited to the herein mentioned authors.

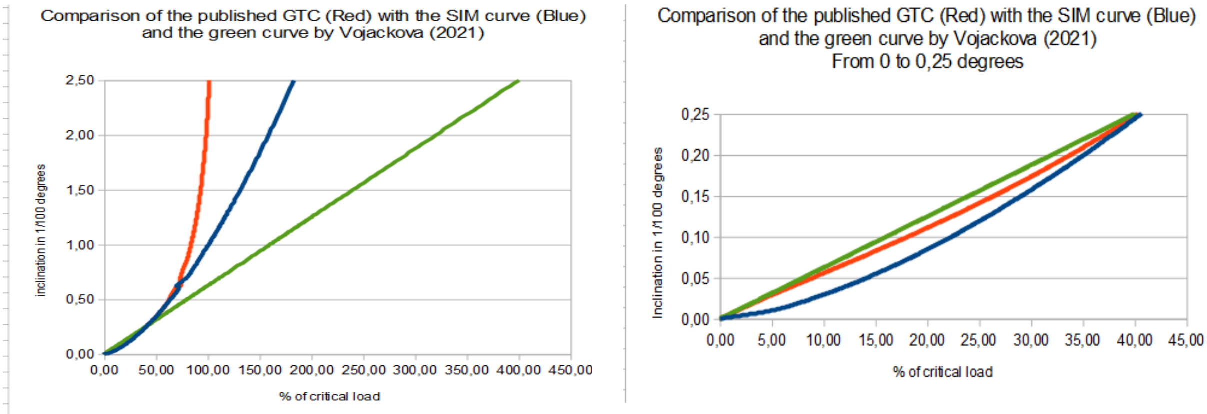
Results and discussion

Perceived discrepancies and contradictions that question the "Generalised Tipping Curve"

The simple mathematical experiment carried out with published values from several *"tipping curve"* diagrams and the 234 inclinometer measurements by the corresponding author, revealed a

very different curve, compared to the published diagram of Wessolly and related authors (e.g. [9-15,18]).
The real *SIM* tipping curve as used by Detter et al. [11] in 2003-2004 seems to be very different from the *GTC* they have published (Fig. 1).

Figure 1: the red "Generalised Tipping Curve" as published by Detter et al. ([11]) is compared with the results of the *SIM* software used by Detter et al. ([11]) (blue curve) and the green curve by [15]. Note how the *SIM* curve lies below the published *GTC*.



And this has serious consequences:

First, 100 % of M_{crit} would be already reached at $\theta \approx 1^{o}$ instead of 2.5º, according to the curve that was calculated from published results and also the results of the *SIM* pulling test software. It is only at $\theta \approx 0.25^{o}$ that both curves cross and give thus a similar M_{crit} and safety factor for uprooting.

And again, it is a bit *odd* that that curve and the "M_{crit} = *100%*" also coincide so precisely, but now at an θ of 1º. The *GTC* is allegedly the generalised result of uprooting 400 trees [12]. So, again, the mathematical precision (M_{crit} = *100%* at $\theta \approx 1^{o}$) is astonishing. One would think that nature-made structures (trees, root systems and soil conditions) would not obey such a constant mathematical precision.

Secondly, this means that the *SIM* formula invariably gives a safety factor of only 55.35 % compared to the safety factor as calculated with the published diagram at high angles (e.g. $\theta = 2.5^{o}$). And, on the other side of the spectrum, at very low angles (e.g. $\theta = 0.01^{o}$), the SIM formula gives a safety factor of only 34.10 % compared to the safety factor as calculated with the published *GTC*. *In other words, the "real" uprooting safety factor of the trees can be as low as one third (1/3) of the safety factor as calculated with the published GTC.*

And the third curve by [15] shows that theirs is *again* very different from the published *GTC* and that 100 % of M_{crit} is reached at $\theta = 0.625^{o}$, which is again different from the former two curves. And thereby, and at the same time, they confirm (with words) and refute (with their tipping curve) the claims of their colleagues, the published *GTC*, the *SIM* curve and their entire Inclinomethod. The authors of [15] used a *FE* model, that was simply a beam fixed on rotating a half sphere which was said to agree sufficiently with the results of their pulling tests. This could suggest that the GTC and SIM tipping curves were developed on that model and not by uprooting real trees.

Hence, the following question inevitably arises now: *Why is the "generalised tipping curve" as published in the diagram or cited by [9-13] from 1998 to 2016 so different from the real curve from the SIM software, and also and the third one (based on one experiment)?*

This means that a tipping curve similar to Eq. 4 was already being used by Detter et al. [11] in 2003, while they have been publishing an entirely different one. Wessolly and Erb [10] kept on publishing the original *GTC*, 13 years later.

Moreover, in Göcke and Rust [18] it is said: *"...the tilt values of 0,2°- 0,3° at 50-60 km/h automatically lead to critical tilt values at gale wind speeds (90 km/h..."* (sic. [18]). And that claim can be reproduced with the SIM curve of 2003 (see Methods). This suggests that the commercial method of Göcke et al. [14] and Göcke and Rust [18] could simply be the unpublished SIM curve as already used in 2003-2004 by Detter et al. [11].

However, the equation published in 1965 by Brinch-Hansen but borrowed and adapted by Sagi et al., (2019, [48]) *also* gives the same result: $M_{crit} = 100\%$ at $\theta = 1°$.

Figure 1. An adapted equation from 1965 gives the same result as the unpublished SIM curve

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Figure 3

The Eq. 12 of Sagi et al (2019) taken from Brinch-Hansen (1965) gives similar results as the SIM tipping curve from 2003, even though related authors were Publishing the entirely different GTC curve of Wessolly, Brudi, Detter and others.

Data input				**Sagi et al 2019. Eq. 12**		
Moment WLA=	144,13	kNm		a=	1,00	
Height cable=	10,00	m		n=	0,68	
Angle cable=	0,00	degrees		angle	1,00	°
Dynamometer F=	5,77	kN pull	**Normalised moment Mo=**		**1,00**	
Pullingtest moment=	57,65	kNm				
Simulated M/ Pred M=	40,00	%		**Sagi et al 2019, Eq 13**		
				G=	11,012	MpA
				b=	0,52	m
				u=	0,5	
			Rotational stiffness=		**8258**	**kN/rad**
					144,13	**kNm/degree**

Published Generalised Tipping Curve (Wessolly, Erb, Brudi, Detter et al.)

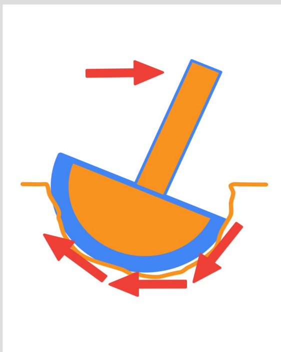
Inclinometer=	**0,25**	**degrees**		
	40,30	% of critical load	**Coeff.**	**Value**
Critical moment=	143,05	kNm	a	0,2794030591
Safety uprooting=	**99,25**	**%**	b	1,1713389998
Crit. Rootplate radius=	1,29	m	c	0,4217422403
			d	114,14344562

Formula deduced from Sterken's pulling tests (SIM)

Inclinometer=	**0,25**	**degrees**
	40,63	% of critical load
Critical moment=	141,91	kNm
Safety uprooting=	**98,46**	**%**
Crit. Rootplate radius=	1,29	m

Correlation suggested by Vajockova et al (2021)=

Incl=	0,25	degrees
Moment=	92,05	kNm
Mcrit= (M/40)*100=	230,13	kNm
Critical angle=	0,625	degrees
Inclinometer=	**0,25**	
Critical moment=	92,05	kNm
	40,00	% of critical load
Critical moment=	144,13	kNm
Safety uprooting=	**100,00**	**%**
Crit. Rootplate radius=	1,29	m



Formula deduced from Detter and Rust (2013)

Inclinometer=	**0,25**	**degrees**
0.25*2.5	0,63	
0.25/2.5	0,08	
	41,14	**% of critical load**
Critical moment=	140,14	kNm
Safety uprooting=	**97,23**	**%**
Crit. Rootplate radius=	1,29	m

The "Crit. Rootplate radius" here above is from Pantaleo et al (2005)

And the unpublished SIM curve from 2003 *also* agrees with an uprooted tree in Detter and Rust [13]. And this could be perceived, not as the result of empirical results from real trees in real winds, but as misleading information. And yet, [9-12] meanwhile published an entirely different "generalised tipping curve", which is odd and might require answers. The uprooting diagram of

([13], Fig. 6) is very similar to their real *SIM* software curve from 2003-2004. The original *GTC* they had always published disappears here mysteriously into the mists of time. It is a striking coincidence indeed that both the tree in ([13], Fig. 6) in 2013 and examples in [18] from 2015 agree with the *SIM* curve from 2003.

The following four findings can be stated now:

- The strong *"t/R found in standing trees"* claim based on strain gauge measurements published by [9-11] was found to be untrue [5];
- Several commercial wind load analysis software packages were successfully simulated with an utterly simple model, among them the *SIM* software of Detter and Brudi (see [4]);
- The *"natural frequency"* from the *SIM* software of Detter and Brudi ([11]) could also be perfectly simulated with a formula for brick chimneys.
- Also the published "*generalised tipping curve"* differs greatly from the curve used in the *SIM* software and the third curve of [15] (assertedly validated by *FE* and uprooting one tree).
- And, are their two tipping curves really the result of *"uprooting 400 trees"* as stated by Brudi and van Wassenaer ([13]) and others? If that were true, then there should not be two completely different tipping curves published (*GTC*) and used (*SIM*) by them, right?

It has become hard to discern the truth from irregularities and contradictions. And these four premises seemingly question, contradict or refute their published claims, so: *How is that possible?*

Examples of the considerable number of published contradictions and seemingly erroneous information, all by authors related to the same *GTC* and commercial pulling tests will be offered here, to show the misleading potential of the related publications. The following analysis unveils more strange coincidences and incongruencies:

In Detter and Rust [13] a striking coincidence was found. They say: *"Während für Eschen (Fraxinus excelsior L.) und Winter-Linden (Tilia cordata MILL.) der Faktor 2,5 eine gute Näherung für die Hochrechnung der Versagenslast darstellte, lag dieser Wert für Berg-Ahorne (Acer pseudoplatanus L.) mit ähnlichem Durchmesser deutlich höher (Faktor 4,5)."* (sic. [13]).
So, their extrapolation for the first two trees, by multiplying *M* at 0.25º by a factor of 2.5 would correspond to 100% of M_{crit} which would be 2.5º for the *GTC* and 1º for their *SIM* curve. But the factor 4.5 for the third tree, *coincidentally* corresponds to ± 183% of M_{crit} which is the exact value given by their *SIM* curve at 2.5º (as 40.63 times 4.5 = 183).
It is very odd that those three trees would yield *exactly* those results.

Detter and Rust ([13], Fig. 9) give two correlations between the moment at 0.25º and the maximum uprooting moment: Wessolly's factor 2.5 versus their results from uprooting 20 trees (y = 3.0x + 14.0). Their prediction would agree (R^2=0.92) with real uprooting moments. But, it exceeds Wessolly's prediction by 183% at 8.9 kNm. And at 50 kNm their M_{crit} is 132% higher than the value predicted by Wessolly. If it really were true that *"0.25º = 40% of M_{crit}"* as claimed by these authors, then both correlations should at least run parallel to each other, instead of deviating from each other. There is a difference of 53.6% within their own linear correlation of measured M_{crit} for 20 trees. Which means that the pulled-down trees do not follow any of the curves. (The lower value of 8.9 kNm at 0.25º was chosen as it gave exactly the same result as their *SIM* curve at 2.5º = 183% of M_{crit}).

Göcke et al. [14] validated their product by stating: *"Standard pulling tests for anchorage rely on the fact that there is a linear correlation between load at a root-plate inclination of 0,25 degrees and load at failure (Detter and Rust, 2013)."* (*sic.* [14]). But Detter and Rust [13] show deviations up to 53,6% within their own correlation, so that should seriously question the reliability of the product of Göcke et al. [14] too.

Detter et al. [37] pulled trees down, to compare the result with published values and stated in 2019: *"The curves resembled the so-called "generalized tipping curve" (Wessolly and Erb 2016)..."*(*sic.* [23]). However, their real *SIM* curve does *not* resemble the *GTC* of [9-12]. So, does that mean that the real *SIM* curve, employed in 2003-2004 by [11] and found in [13] would then be *wrong?*

And Rust and Detter [38] say: *"...measuring the tree's reactions with a high-precision inclinometer, and extrapolating those data to determine the minimum strength of the root system (WESSOLLY, 1989; DETTER and RUST, 2013; ..." (sic.* [38]). But, Wessolly uses a different correlation than Detter and Rust [13] to determine that strength and with different results.

So, which of the two is reliable now?

And: *"Using the method described in WESSOLLY and ERB (1998), there was a good correlation between estimated and measured anchorage strength (Fig. 1), although anchorage strenght was systematically understimated."(sic.* [38]). But, as shown herein, the correlation of Wessolly and Erb [9-12] greatly *overestimates* anchorage strength as it underestimates M_{crit} (1º = 84% M_{crit}).
And their *GTC* overestimates the angle at which M_{crit} is reached (2.5º), compared with the real *SIM* correlation of [11] and the one found in[13] (1º = 100% M_{crit}) and Göcke and Rust [18]. Does that mean that [38]) then refute their own *SIM* curve they used in 2003-2004 and published in 2013?

And surprisingly, Vojáčková et al. [15] *also* publish an entirely different curve.

This is all very incongruous and confusing.

Dellus, Wessolly and Detter [39] state: "*Other parameters like tree natural frequency and damping coefficient are used to calculate a "gust reaction factor", which expresses the dynamic response of trees to gusts." (sic.* [39]). Their claim was seriously questioned in [4] and could now be perceived as misleading information, as the "brick chimney" formula simulated perfectly their *"tree natural frequency"*. And: *"The safety factors for the root system and the trunk are calculated by extrapolation of measured values to high winds. Reference values for green wood properties and the general tipping curve allow calculate fracture and uprooting loads (Wessolly and Erb, 1998)." (sic.* [39]). But that quote could clearly be perceived as misleading information, as in 2003 they had already used an entirely different SIM curve.

Buza and Divós [40] show a diagram of what looks like an uprooted forest tree that would follow *precisely* the published *GTC* of Wessolly, Brudi, Detter [9-12],... But then, obviously, their diagram does not agree with the *real SIM* curve anymore.

Also Rinn [41] says: *"Our measurements indicate that Wessolly's hypothesis probably was a good guess."* and *"...with a deviation in the range of ±20%. In other experiments, the estimation error was bigger, in some smaller."*. Rinn [41] published several diagrams to support his findings and he thus more or less agrees with the published *GTC* (100% M_{crit} at 2.5º), but which shows here *not* to be the real *SIM* curve.

This is all very irregular and confusing: *Which of the curves is correct now, if any?*

Hence, several conclusions can be drawn from such strange findings:

First, research bias could play a role here, otherwise these researchers would not have published uprooting data that, strangely, "confirm" and "validate" three very different curves. And if any of these tipping curves were really reliable and really the result of pulling trees down, then the uprooting experiments should only support one curve, and not three very different ones.

Next: All these authors state that 0.25º = 40% of M_{crit} (M_{crit} = 2.5*M at 0.25º). If this is really true, then their extrapolations would only work fine as long as the tree is pulled (or pushed by the wind) to exactly 0.25º. However, at lower or higher angles (e.g. 0.06º and + 2.5º) the extrapolations then become very unreliable, as each gives a different uprooting safety factor.

A good example of how flawed or dubious leads can channel our minds towards erroneous directions is [15] published in a reputable journal which guest editor (G. Dahle) has also co-authored with several of the herein investigated authors. In it, a finite element (*FE*) model of the tree response to static loading was validated with pulling a real tree over [15] . The authors amply refer to the inclino-elastomethod of Wessolly, Brudi, Detter, *etc.* and perform the same method on that tree. So far, so good... Nevertheless, there are several crucial remarks to be made:

First: They victoriously state: *"The presented FE results and experiment (Fig. 5a) confirm that inclinometers are a reliable tool as stated by Wessolly and Erb (2016) and Detter et al. (2002)."* (sic. [15]). However, nowhere in their paper can a validation of the *GTC* of "*Wessolly and Erb (2016) and Detter et al. (2002)" (sic.* [15]) be found. And that *GTC* now appears to be very dubious. So, their statement can be misleading.

Second: they do pull a tree over with the inclinometers, but they do not report if that uprooted tree had obeyed the *GTC* or not. It would have been good if they had not omitted that result.

Third: They say *"...confirms that extensometers are a valid tool for local measurements of sound wood properties"* (sic. [15]), which is possibly true. But then, they improperly extrapolate the former sound finding and then claim: *"The extensometers capability to sufficiently reveal defects at the same position is confirmed..."* and *that* can also be misleading. In their *FE* model, simple beam theory was combined with a 90% reduction of the modulus of elasticity (*MOE*), to simulate structural defects. And that approach is not reliable for many types of common structural defects and behaviours, as any engineer should know (see also [3-6]).

Fourth: The entire crown had been cut down by [15] and its influence on the critical tipping angle and gravity-based moments was thus also neglected in their pulling tests.

But, notwithstanding, here comes some conclusive evidence: The *FE* model of [15] was a beam fixed on rotating a half sphere. Their model was said to be validated, as the correlation predicted moment versus inclination, for both the *FE* model and a real uprooted tree: *"...the overall character of root-plate deflection (Fig. 6) is comparable between model and measurement."*(sic. [15]). And they thus conclude: *"The presented FE results and experiment (Fig. 5a) confirm that inclinometers are a reliable tool as stated by Wessolly and Erb (2016) and Detter et al. (2002)."* (sic. [15]).

Their "validated" tipping curve predicts that $\theta = 0.25$º would equal a *M* of 92.05 kNm. And according to [9-13] that angle would represent 40% of M_{crit}. Hence, it follows that M_{crit} would be 230,13 kNm. So far, so good... Nevertheless, [9-12] also say that 100% of M_{crit} is reached at θ =2.5º. Which, according to Eq. 5 would then be 920.5 kNm. And this is now 400% higher than predicted at $\theta = 0.25$º (230,13 kNm). This simply means that the correlation of [15] already refutes the claim *"2.5º = 100%M_{crit}"* of Wessolly and others [9-13]. And according to the real *SIM* curve (*"1º = 100%M_{crit}"*) M_{crit} would be 368,2 kNm. Which lies closer to the result of Eq. 5 but it is still 160% more.

So, and as is shown here, the authors of [15] at the same time confirm (with words) and refute (with their tipping curve) the claims of their colleagues, the published *GTC*, the *SIM* curve and hence the entire Inclinomethod. Even in 2021 and in a reputable journal, dubious claims from 1998-2016 are still being perpetuated. Dubious leads can channel our minds towards erroneous directions, while time and funding are suboptimally spent.

The original and published *GTC* seems to greatly overestimate uprooting safety and may thus lead to the unexpected uprooting of “safe” trees. Perhaps it would be recommendable to restrict the pulling tests to scientific research only, so as to reach a perhaps higher predictive value in the future, *before* commercialising them. Other arboricultural companies have developed and marketed their own brand of pulling test (e.g. [41]), in the wake of [9-14]. Hence, if those companies have based their pulling test on the latter's *GTC*, then they should really be worried now.

In several publications, the unveiled tipping curves and extrapolations are said to come from pulling trees over. Or examples of real uprooted trees are given that assertedly “validate” their curves. However, the the great amount of “coincidences” and “validations” and incongruencies could be perceived as, quite simply, misleading information, research bias or errors.

And the forest tree pulled over in Sagi et al (2019, [22]) clearly has an ideal and modelable *“..rigid surficial circular foundation..” (sic* [22]) and does not seem to be affected by decay or other defects as commonly found in urban environments. Hence, extrapolating such results to urban trees and their common mechanical defects, limited root space and decay, and thereby “validating” the pulling tests is a questionable procedure.

Rinn ([41]) proclaimed: *“Our measurements indicate that Wessolly’s hypothesis probably was a good guess.”* and *“Wessolly’s hypothesis was a good guess, however, the data we obtained did not allow us to really confirm the concept.”* (sic. [41]). However, his article did not offer any references to a peer-reviewed journal where he would have proven that claim or published the results of his own commercial brand of pulling tests. But the present findings suggest that also Rinn ([41]) was misled and, consequently, also published questionable statements.

Brudi and van Wassenaer (2002) also victoriously proclaimed: *“So far, the inclino method is the only method that provides reliable information about the anchoring potential of a tree.”* And: *“...provide a scientific approach to tree failure analysis based on sound engineering principles.”* (sic. [12]). Their proclamation neither looks sound nor scientific anymore, and should perhaps be revised.

In 2016, Wessolly and Erb stated: *“...it is estimated that far more than 15.000 assessments have been carried out...”* and *“10.000 of these assessments can be found in our archive.”* and *“It is used in court proceedings as evidence...”* (sic. [10], p. 230). Those court proceedings should perhaps all be revised now. Some of the herein investigated authors are court-certified expert witnesses who provide the court with a neutral assessment based on their knowledge as experts against the background of certain specified facts.

One could thus also wonder how many pulling tests have wrongly-assessed trees, based on the diagram, over the last three decades and all over the world. Or consider improper evidence adduced in court in front of a judge, deviating the course of true justice (see also the remarks on *VTA*).

The lack of supporting scientific evidence adduced by the founding authors in [9-12] and the efforts made by related authors to justify and validate their pulling tests with very few and even incongrous and contradictory examples, suggest a great fragility of their method.

Perceived discrepancies and contradictions about dynamic and "quasi-static" wind load analysis

Mathematical results

The comparison with **the equation for stiff brick and steel chimneys** from [6] reveals a surprising result:
That formula combined with a simple value (4420) gives **exactly the same** "natural frequency" as had been "predicted" for a large number of trees with **WLA and SIM pulling test** software packages. The WLA and pulling test files were shared with the peer-reviewers to ensure the correctness of this surprising finding.
Hence, the results offered herein and published in Sterken [4] suggest that the "dynamic wind load analysis" from the same group of authors is not *"the result of decades of scientific research"*, but from simple textbook equations and that could be perceived as misleading information.

A discussion on dynamic and "quasi-static" claims

The paper of Jackson et al. 2021 [42] possibly inaugurates the first mega-collaboration of a great number of leading scientists in forestry who cooperate and jointly publish with people from urban arboriculture. The motion of trees in the wind was investigated therein, based on large sets of data obtained with static pulling tests, strain gauges, inclinometers and accelerometers, leading [42] to proclaim: *"This suggests that, contrary to some predictions, damping or amplification mechanisms do not change dramatically at high wind speeds, and therefore wind damage risk is related, relatively simply, to wind speed."* (*sic.* [42]).

The amount of data, statistics and calculations described in the painstakingly-crafted paper of [42] is overwhelming and impressive. But, there is an important detail missing. And so, it would be constructive if the following questions could be answered by [42]: *How were the investigated trees selected?*

The selection of trees influences the results. It is known in science that unconscious bias can influence the design of the experiment when a certain final result is *a priori* desired. In other words: some trees can unconsciously be chosen to render the desired results afterwards.

For instance, if I wanted to confirm my hypothesis/suspicions that dynamics and swinging are a crucial component in tree and palm risk assessments, then I would choose e.g. 30-metre-tall *Washingtonia robusta* palms, large and lion-tailed *Pinus pinea* trees or a left-over *Fagus sylvatica* on a building plot (formerly a forest) that has been entirely cleared (that tree would thus be a tall, top-loaded tree, with a crown high up the naked, and slender, trunk and main stems.

And if, on the contrary, I wanted to prove that "my static *WLA* software is reliable" (or that "*..wind damage risk is related, relatively simply, to wind speed* (*sic.* [42])*"*, I would certainly avoid the aforementioned trees. And, I would carry my experiments out on open-grown trees with stiff stems and, *especially,* wide and well-distributed branches and crowns that would efficiently dampen oscillation and structural behaviours and, hence, barely swing. Such as e.g. veteran urban and open-grown trees with well-distributed crowns from top to bottom. That is: the typical open-grown (heritage) trees in urban areas that are commonly selected to undergo the pulling tests. Or the sturdy *Eucalyptus teretecornus* and *Araucaria cunninghamii* investigated and included in [42] by one of the co-authors, James [43].

The following sentence from [42] also seems to support my analysis: *"For example, a small fast moving conifer would be less predictable, and a large broadleaf with complex branching architecture may be less affected by changes in wind loading and therefore be steadier and more* *predictable..." (sic.* [42]). And also *"sheltering effects"* are mentioned: Could those not have contributed to less turbulence and lesser swinging of the investigated trees?

Next, their proclamation *"This suggests that, (...) wind damage risk is related, relatively simply, to wind speed."* (sic. [42]) seems to suggests that we should not worry about damping or amplification mechanisms and that, consequently, the *PT* and *WLA* products of the co-authors would be suitable and recommendable for tree risk assessment.

Also, *"This result could be an important contribution to estimating wind damage risk..."* (sic. [42]) is indeed a very suggestive and optimistic proclamation when many co-authors market the same devices as used for that study. And the following statement could also be perceived as convenient for some of the co-authors who commercially offer strain gauges, inclinometers and pulling tests: *"We recommend the use of strain gauges to measure tree motion in this case since they provide comparable absolute values which can be related to the risk of mechanical damage."* (sic. [42]).

The authors of [42] seem not to be informed yet that the use of strain gauges and the simplistic *BT* theory behind the pulling tests have already been seriously questioned elsewhere (e.g. [3-8]) and that *BT* may not represent the structural failure of biological structures.

It is easier for researchers, who own or use such devices commercially and who also know how to use them, to carry out experiments and publish research based on their devices.

Omitting the necessary description of how the trees were selected may have been an unconscious mental procedure, but it sheds a shadow of doubt on their otherwise painstakingly-crafted and complex paper.

A co-author of [42] cites his paper from 2017 [44] which publisher fitted the definition of predatory as alleged by [45]. That paper [44] reveals that crown architecture was seemingly not taken into account in the final results: *"Trees were selected to cover a broad range of heights (h), widths (diameter at breast height, DBH), and wood densities (ρw)."* And: *"Note that these (...) do not include the crown".* And: *"Broadleaf trees have a more complex crown structure and their sway is therefore characterized by a less dominant main axis. Additional data on the crown architecture of the trees used in this study could therefore explain the differences..."* (*sic.* [44]). These quotes, published by several of the co-authors of [42] also seem to support the need for scrutiny of their claim: height and diameters were consciously chosen (and thus influencing the measured stiffness and sensibility to swinging). But, above all, crucial factors such as crown architecture and crown weight seem to have been neglected in [44].

Co-authors of [42], Rust, Göcke and Detter [46] "validated" their commercial *WLA* software, based on only four trees (*n* = 4, and still reporting differences up to 32% between predictions and measured loads). This overrating of results based on a very small trial is questionable. But from what shines through their under-explained Methods section is that at least nine trees were investigated. So, why were the results of the other five trees left out?

Co-authors of [42] from forestry are e.g. Gardiner, Achim and Ruel who support a mechanistic model named ForestGALES [33] which is simple beam theory and static *WLA* in which no oscillations or dynamics are taken into account [34].

However several weighty contradictions were found too:
Gardiner and others stated in 2016: *"The model by Virot et al. [1] is static. However, previous comparisons between static models and experimental data have revealed that dynamic effects linked to wind turbulence (wind gusts) cannot be neglected."* (*sic.* [32]). And yet, in 2021 the same

author wipes that suddenly off the table in [42]. Also in 2019, Jackson and co-authors of [47] literally state: "*The existence of this damping mechanism demonstrates the relationship between wind damage risk and tree architecture: trees with certain architectures will dissipate dangerous sway energy more efficiently and so reduce their risk of damage in storms." (sic.* [47]). And yet, in 2021 the same authors inexplicably swipe that under the carpet in [42]. Which is highly incongruous.

Another co-author of [42] is Newson who appeared in Sagi et al. [48], and in which the *GTC* of Wessolly, Brudi, etc., is cited. One tree on a tree farm was pulled over and the description suggests that both the root system and stembase were mechanically undisturbed and sound (i.e. no structural defects) before the experiment. Van Wassenaer and Detter also cooperated in [48]. Sagi et al. [48] state: *"The values (...) fall within the values reported in (...) Wessolly and Erb, 1998; Brudi and van Wassenaer, 2001 (...)" and "The bending moment and shear force responses of the roots were qualitatively the same as those found by other researchers"* (*sic.* [48]). And: *"Sinn and Wessolly (1989) published a 'generalized tipping curve' using the data obtained from destructive winching tests of 400 trees. (...) the method gives quantitative information on tree stability (...)."* (*sic.* [48]). A rigid surficial circular foundation from classical elasticity theory was said to be consistent with their results and their "tipping curve" gives exactly 100% M_{crit} = 1º (Fig. 7 in [48]). Their tipping curve was based on a formula published in 1965 and is oddly similar to the *SIM* curve from 2003 as discovered herein and which differs greatly from the *GTC* they support. Another issue is that pulling tests are generally used on urban, valuable and structurally-damaged trees for consultancy services. Hence, perhaps speciments with damaged, severed and decayed root systems should be pulled over, instead of sound and undisturbed ones from a "tree farm" or a forest, if they want to validate the *PT* for *damaged* trees. The selection of trees influences the results. It has been shown that researchers are more likely to report a product to be effective if they are funded by the sponsor that has financial interest in that product [49].

Next, Jackson et al. [42] and some co-authors belittle the dynamic nature of tree motion and oscillation in earlier papers:

The authors of [50] write: "*This approach accounts for the dynamic nature of wind loading, but does not explicitly account for the dynamic nature of tree motion". (sic* [50]).
And*:"This means that a large, heavy crown substantially increases Rgrav" (sic.* [50]). But, then they say: *"Our results demonstrate that (...) the effect of gravity is secondary.." (sic.* [50]). And so, they draw their conclusion from a wind speed of only 12 m/s... However, they also say: "*The gravity contribution increases with wind speed, since the tree deflection is higher and so the overhanging weight of the crown causes a greater moment"* (*sic.* [50]). Hence, can the former statement (based on 12 m/s) then really be extrapolated for higher and destructive wind speeds?

Then they acknowledge: *"We found that accounting for the mass of the tree crown substantially reduced the maximum predicted height and increased the gravitational risk factor (...)."*(sic. [50]). And then they state: *"...we extrapolate beyond the range of wind speeds observed in our study (...). This entails the assumption that the wind–tree relationship remains linear and cannot account for the potential effects of streamlining (...) or dynamic amplification or damping of tree motion (...) (sic.* [50]). And: *"...analysis did not explicitly include the complex drag forces (...) and therefore likely underestimated the wind loading on the crown."(sic.* [50]). This suggests that they actively excluded the dynamic nature of tree motion and oscillation from their research.

A methodological error could also have influenced the findings of [42]: Their pulling tests apply a small load and measure the strain, which is then extrapolated linearly, based on Hooke's law (*ut tensio sic vis*), to predict loading at a higher wind load. This is simple beam theory, governed by two premises: that strain should be linearly proportional to stress and that the curvature of the beam should be small enough (*i.e.* a quasi non-deformable stiff column). Nevertheless, slender and flexible stems may exhibit a high curvature and large nonlinear deformations, whereby strain is thus nonlinearly correlated to (wind) loads [3]. In the supplementary data of [42], *very* slender stems were found, with height/diameter ratios as high as 177 (7.8/0.044m) (*e.g.* Guanica, Puerto Rico, 2007 Broadleaf forest, 9, Strain gauges 4). And it is not unreasonable to suppose that such slender stems may exhibit a high curvature and deformation and, hence, strain that is non-linearly correlated to high loads. Therefore, if strain was measured on tree 9 (Guanica, Puerto Rico) with a pulling load that simulated a bending moment at e.g. 4 m/s, then linearly extrapolating the first could result in *underestimating strain, deformation and swinging* at 32,5 m/s (hurricane force). Hence, the proclamation of [42] which apparently belittles amplification mechanisms, swinging and oscillation, may be, up to some degree, fallacious and based on methodological errors.

Another cited paper from one of the co-authors (Schindler, [51]) also reveals what looks like a shortcut made to support their statement: *"This paper deals with the dynamic responses of three plantation-grown Scots pine trees to turbulent wind loading"* based on *"...hourly mean wind speeds at canopy top below 4 m/s−1)..."* (*sic.* [51]). And*: "For the analyzed wind speed range it is shown that most of the energy absorbed by the trees from the wind is dissipated by tree sway in the first mode" (sic.* [51]). Can the results obtained from a light breeze (4 m/s) really be extrapolated, and beyond a reasonable measure, thereby neglecting the influence of mass, gravitation and acceleration of tree crowns bent over in *destructive* winds?

And another cited paper from Schindler states the following (Schindler and Mohr [52]): *"The decreasing importance of the oscillatory components suggests that wind loading in the range of the damped fundamental sway period of the trees is inefficient and insignificant for total tree movement under non-destructive wind conditions. Consequently, there was no evidence of the occurrence of resonance effects between wind and tree response. (sic.* [52]). Here, the interesting phrase is: "*...under non-destructive wind conditions...".* And yet, such statements built on "*...recorded on a windy day"* (*sic.* [52]) are catapulted towards the statement of [42], to support pulling tests and wind load analysis software that purportedly predict the stability of trees under *highly-destructive wind conditions*.

As has been stated, researchers are more likely to report a product to be effective if they are funded by the sponsor that has financial interest in that product [49]. So, a balanced approach of this issue seems to be justified.

Pulling tests and swinging

A video can be downloaded (Supplementary Material 3) that was recorded by the corresponding author, from the window at the office of Brudi and Partner Treeconsult ([11]) in 2002. It shows how a Norway spruce tree (*P. abies*) quite clearly swings in a storm, causing horizontal displacements, even if side branches may damp part of the energy. Simple observation may contradict biased research results (e.g. "dynamics are not important" or "trees are quasi-static").

A word on Visual Tree Assessment

According to the drawings by Mattheck in [59], inner decay and bending stress would allegedly lead to visually easily detectable folds and "macroscopic buckling symptoms" on the outside of trunks, at root flares and the underside of branches. That suggestion was, as a business model at least, brilliantly linked to his commercial Fractometer in several of that author's publications. That Fractometer (marketed also by the author of [16,59]) was once a hugely popular device that allegedly allowed for assessing the breaking safety of a tree by measuring the radial bending strength of 5-mm-wide cores taken with an increment borer from the residual wall. Although anyone with enough common sense could also have taken an extracted core, broken it radially by hand at the junction of the annual growth rings and wondered: *"Do trees really break this way?"*. Those devices have been successfully marketed, together with related workshops and books, based on their axiom.

The authors of [16,59] also asserted that branches with included bark would push each other outwards, leading to possible fracture. Logical reasoning was offered in [7] that shows that those two ideas, when combined, appear to be self-invalidating. The evident contradiction was noticed by the corresponding author in 2002, in a Spanish arboricultural congress, when prominent businessmen tried to sell these two untenable suggestions, together with related commercial decay detecting tools, in their workshops and lectures. The "compression fork" and other fairytales refuse to dissapear as they are still desperately clung on in Spain by treeclimbers, business people and university professors alike.

And, as with their "axiom of uniform stress" their suggestion *"folds and macroscopic buckling symptoms"* is hard to prove empirically. One should thus wonder if such suggestions – that can hardly be proven either right or wrong – should belong to the realm of real science.

Nevertheless, those folds can easily be observed in even sound e.g. *Platanus x hispanica* or *Morus nigra* trees, and regardless of decay or "bending stresses", as this is just a common feature of those tree species. The corresponding author observed how those questionable criteria were applied to hundreds of monumental plane trees in Aranjuez, Spain, in 2000 by an arboricultural consultant. And as the latter's bank account grew bigger, the trees were made smaller. Many were either cut down or had severe crown reductions, and all based on an untenable t/R rule, drilling, boring, Fractometer and... wrinkles. These questionable drawings and criteria have been used recently in court cases too (e.g. [60]). And yet, if one observes the annually pollarded plane trees (e.g. in an umbrella shape to provide shade) in Southern European villages and cities, one could look at those wrinkles and think: *"Are these trees, with barely any crown or loads acting upon them, really under that much mechanical stress?"*. And then, common sense debunks famous, but erroneous, claims and criteria that have been clung on for decades.

Also, their influential t/R=0.32 rule was recently scientifically refuted in [3] while it has been attributed to falsification (Grüber in [3]). This, hence, could question the validity of their entire Visual Tree Assessment methodology.

Irreproducible claims

Green [61] calculated critical wind speeds for breakage of Eucalyptus trees with the engineering approach ("tree-statics") citing Brudi ([12]) and James et al. ([43]). Eucalyptus tree Nº5 is described in her thesis as having a surface area of 51 m² (Table 3 in [61]) and then 16.5 m² (Table 5 in [61]), which is confusing. However, I calculated the surface of tree Nº5 (Figure 42b in [61]) with a special software and got 33.59 m². So, the three different vertical surface areas for the same tree made it impossible to reproduce her results. And unfortunately too, Figure 42B in [61] was the only silhouette available in the thesis that was identified with a tree number and that would have enabled reproduction of her results. I sent an email with this observation to the Research Ethics and Integrity team of the RMIT University of Australia. Green's supervisor replied that he had not been able to contact her and that neither he nor the two examiners had spotted the errors in her thesis, wishing me luck with my paper on Research Integrity. Hence, the main findings in Green's thesis are irreproducible too, which seems to be quite common in the herein investigated publications.

A word on “My method works!”..., or fallacious reasoning in arboriculture

Some people claim and publish that “their method works as the trees or palms that they have left standing are still there”.

In an answer to my paper published by *Scientific Reports* [3], an arboricultural consultant wrote me that his self-developed method would be “reliable” as the date palms that he had assessed (and left standing) had already withstood winds up to 100 km/h, suggesting that unexpected failure afterwards was non-existent and that his method was thus “reliable”.

Already in 1998 Wessolly suggested that their method was infallible: *“Sie haben sich bisher in mehreren europäischen Ländern bei über 3000 Baumgutachten bewährt, ohne dass eine Fehldiagnose bekannt ist.”* (sic. [9], p. 169). And later, Wessolly and Erb ([10], p. 164) also victoriously proclaimed that: *“We have applied the safety factor 1.5 for 25 years for 10.000 trees without any damage”* and *“We can even offer evidence: the trees recorded in the example of the diagram have survived all hurricanes which occured in the meantime without any problems. Thus, the measured values have proved themselves in practice”*.

This is just a small sample of people who claim their methods to be reliable, but this statement can be heard from arborists and tree consultants too.

But, can it be proven that the “perhaps unsafe” trees/palms that have been assessed and left standing (with a crown reduction) or felled, would have broken/uprooted if those “reliable” methods had not been used and the trees left untouched?
The answer is obviously “no”, because the assessed and referred to trees/palms, have either been pruned, cabled or cut down entirely. Thus: the necessary evidence that could prove their methods wrong or right has been eliminated. And so, such a statement has no value: it proves nothing and it cannot be proven. An impossible solution would be to live in two different but parallel realities: in the first the trees could be assessed with those methods and then cut down or the crown reduced, and in the second the trees could be left untouched and one could record if they would fail during storms, or not. In this way the (un)reliability of their methods could be proven by comparing the outcome. And true, this option is clearly not possible... But, if a statement cannot be proven, then that statement has no place in science and is thus simply conjecture or pseudoscience.

Hence, commonly-used statements such as these are a fallacy, produced by the human tendency to “prove” something through bias, *a priori* desired results and fallacious and circular reasoning. By no means is saying *“my methods work because the trees are still there”* any supporting evidence at all. Moreover, these risk assessment procedures are strongly governed by bias. It is even not unthinkable, that among e.g. 100 trees only the ones are chosen that would either fit the flawed theories or that would give the desired final results (desired result: e.g. the exclamation “*My method works!*”).

In short: this “positive self-validation” could simply be the result of fallacious and circular thinking as a product of bias. First, visual assessment, bias and often dubious criteria are used to select the specimens to be assessed with further inspection (e.g. drilling, tomography, pulling tests,...). Then, their questionable methods come on stage which are often opposing and have been the scene of acrimonious debates in the industry (e.g. Mattheck's *VTA* and drilling versus Wessolly, Brudi and Detter's pulling tests: see [6] for an analysis on this subject). And afterwards, the assessed trees are either cut down or recieve crown and branch reductions (often as much as 15-35% of the total height is cut away) and cabling. And when a storm has passed by they claim then that “their method works, as the trees that they have not condemned to felling are still standing”.

Several influential *VTA* ideas published in [16,17,59,63] are, even though apparently fallacious, very smartly constructed, as they cannot be proven right or wrong in a conclusive manner (except for their *t/R* rule as it was a numerical value). And so, in order to defend and "validate" their refuted *VTA*-failure criteria in a scientific journal, Mattheck and Betghe state for instance: *"We ask the arborists to use their common sense and if concerned to go in the woodlands to check who is wrong and who is right."* (*sic.* [63]). Which is suggestive of pseudoscience and fallacious reasoning to "validate" a method that had already been refuted scientifically.

And, if two entirely different methods are used world-wide (e.g. pulling tests versus drilling and *VTA*) and, yet, each related supporter claims that "their method works", then there is something wrong with that equation.
Either only one or none can be reliable, but not two methods whose premises, founders and researchers have clashed ever since. And if we left current commercial tools and methods out of the equation and just leave bias, visual assessment, cutting down, pruning and cabling, then the final result might well be exactly the same: the same trees are left standing and the same actions undertaken, but it wil be a lot cheaper. So, and starting from the premises that these tools and methods may not be reliable and that the assessment is, either way, based on bias, one could wonder if those questionable methods should then simply not be left out of the equation. The final outcome would possibly be the same: either a 5-30% crown reduction and cabling plus periodical assessments and maintenance or felling. Private clients and city councils would save a lot of money and the trees would probably thrive as well or as badly, and face the same fate as before.

A short word on The Black Swan Theory
Pseudoscience would say: "We have never seen any black swans, so all swans are white".
And real science says: "The observation of only one black swan refutes the hypothesis that all swans would be white". In other words: a theory has to be abandoned or modified if *only one* single observation disagrees with the prediction of that theory. Examples could be: *"I have never seen a broken palm tree, so palms never break!"* or *"My method works, because I have never seen a failed tree after I did my thing!"*

Conclusion
The amount of papers supporting "quasi-static" *WLA, PT,* decay detecting tools and *BT* has become baffling over the last decades, and related authors are very often directly related to corresponding commercial tools and firms. This unbalanced prevalence may be due to research and publication bias where everyone seems to follow a potentially unstable and flawed lead (e.g. the *GTC, BT, VTA, WLA*).

Unfortunately, the historical path of the pulling tests is, since 1998 to now, seems to be strewn with contradictory statements and even self-invalidating findings. Reproducible results and data transparency regarding claims such as the *GTC, VTA* or *WLA* are generally lacking, which makes testing their asserted validity almost impossible. And up to some considerable degree and depending on the case, symptoms were found that are suggestive non-rigorous research, methodological errors, bias or misleading information.

Other examples suggest shortcuts, inappropriate extrapolations, feeble argumentations and experiments unconsciously driven by an *a priori* desired result, leading to victorious proclamations evolving around proclamations similar to: "oscillation and dynamics are not that important" and "our mechanistic forestry models and commercial WLA and pulling tests are reliable". Or *"The results obtained by the inclinometer sensor show that around 2.5 ° of inclination trees have the yield strength. At this particular stress the failure of the root anchorage occurs. As argued by Wessolly (1996), ..."* (sic. [53]).

Giambastiani et al (2017, [53]) carried out pulling tests on trees that *"...did not show significant defects that could compromise stability"* (sic. [53]). They modelled the root system as a root plate area of a hemispherical shape. And they state: *"In agreement with other authors (Wessolly, 1996), at the 2.5° threshold the break of the soil-root system occurs, as the failure of the root anchorage"* and *"For the model building, the force to tilt the root plate of 2.5° is used as the force needed to overcome the root anchorage"*. Then they conclude: *"The results obtained by the inclinometer sensor show that around 2.5 ° of inclination trees have the yield strength. At this particular stress the failure of the root anchorage occurs. As argued by Wessolly (1996), ..."* (sic.) and *"The used value (Low Load = 23% High Load) is comparable with the values of the "General Tipping Curve" of Wessolly."* (sic. [53]).
But, if the *"General Tipping Curve of Wessolly"* has proven herein to be **very different** from the *SIM* pulling tests curve, and related authors suggest that 1º is needed instead of 2.5º, then that means that Giambastiani et al (2017, [53]) have also published very confusing and incongruous information when "validating" the *GTC*.

And the forest tree pulled over in Sagi et al (2019, [48]) clearly has an ideal and modelable *"..rigid surficial circular foundation.." (sic* [48]) and does not seem to be affected by decay or other defects as commonly found in urban environments. Hence, extrapolating such results to urban trees and their common mechanical defects, limited root space and decay, and thereby "validating" the pulling tests is a questionable procedure.

Secrecy in research (*i.e.* not adducing equations, evidence or supporting data or obscure citational practices and contradictory claims by the same group of authors related to the same method) perpetuates misbehaviour and breeds mistrust [62].

Furthermore, alleged predatory journals as flagged by e.g. [45,64,65] can seemingly spread false findings and perpetuate already refuted ones. For instance, a hilarious bogus paper was published in an alleged predatory journal, proving that peer-review was non-existent [65]. Researchers should beware of being perceived as examples of these quotes: *"...ethically reprehensible academic misconduct that 'strikes at the heart of scientific research", "...false or misleading information...", "...dilution and distortion of evidence..." and "...citation cartels" (sic.* [45]). Publishing in predatory journals could lead to establishing or sustaining risky or untenable claims.

Of late, a considerable number of authors supporting commercial tools have amply appeared in a considerable number of articles and journals of dissimilar prestige. Unfortunately for those authors, publishing in an alleged predatory journal could *a priori* cast a shadow of doubt on their claims, their scientific credibility and robustness and their goal (*"Is this science or marketing?"*). A high prevalence of articles was found, even in reputable journals, that perpetuate or support misleading, questionable or incongruous information and that often revolve around the same clubs of authors and editors and the commercial products that some market.

From 2013 onwards several authors have given examples of how trees had uprooted according to the *SIM* curve (*i.e.* M_{crit} at 1º) without explicitely publishing the *SIM* formula (e.g. [13,18]). At the same time, authors such as Buza, Giambastiani et al. and Rinn [40,41,53] show uprooting diagrams or results that "validate" the *GTC* curve (*i.e.* M_{crit} at 2.5º). And then, equations from as early as 1965 by comparatively unknown researchers are published in Sagi et al (2019, [48]) and, coincidentially, also give the same result *(i.e.* M_{crit} at 1º) as the *SIM* curve.

Anyhow, here is a fitting remark: Any crucial statement in a paper that relies on external sources of information should use a citation (unless e.g. newly presented results). Citation of derivations of original work (e.g. *"validating" by citing an article that cites another article that cites..., until the* *original source gets lost in the mists of time*) should be avoided as this practice makes the laundering of dubious claims easier.

Also a funny quote was found, *"rule Nº5"* from Murray [73], that may end this paper with a fitting touch of humour: *"In referring to the previous literature, be careful to cite only the papers that make claims that would support your own, especially those that contain little evidence for the claim, so that your paper shines in comparison"* [73].

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References

1. Gordon, J. E. 1978 *Structures. Or Why Tings Don't Fall Down* (Penguin Books).
2. Not applicable.
3. Sterken, P. 2021 The quest for a unified theory on biomechanical palm risk assessment through theoretical analysis and observation. *Sci. Rep.* **11,** 22134 https://doi.org/10.1038/s41598-021-01679-4
4. Sterken, P. 2021 On trees and wind turbines. *Arboricult. J. Int. J. Urban Forestry* https://doi.org/10.1080/03071375.2021.1903239
5. Sterken, P. 2018 Tree risk assessment: A review of methods. *Arb. Mag.* Spring Issue **180(1),** 40–43. https://doi.org/10.31219/osf.io/4v95b
6. Sterken, P. 2006 Prognosis of the development of decay and the fracture-safety of hollow trees. *Arboricult. J. Int. J. Urban Forestry* **29(4),** 245–267. https://doi.org/10.1080/03071375.2006.9747466
7. Sterken, P. 2022 Letter on Compression Forks and Axioms. In press.
8. Sterken, P. 2022 Letter on Research Integrity in Arboriculture and Urban Forestry. In press.
9. Wessolly, L., & Erb, M. *Handbuch der Baumstatik und Baumkontrolle*. Berlin-Hannover, Germany (Patzer Verlag, 1998). (ISBN 13-9783876170930)
10. Wessolly, L. & Erb, M. *Manual of Tree Statics and Tree Inspection* (English edition). (Patzer Verlag, 2016). (ISBN 978-3-87617-143-2)
11. Detter, A., Brudi, E. & Bischoff, F. Statics Integrated Methods: results from pulling tests in past decades. In *La Visión del Árbol: Actas del 9ª Congreso de Arboricultura* (Ed. Mariano Sánchez García) 103-112. (Valencia, Asociación Española de Arboricultura, 2005). Retrieved from http://www.tree-consult.org. Accessed 3 November 2017.
12. Brudi, E. & van Wassenaer, P. Trees and statics: Non- destructive failure analysis. In Tree Structure and Mechanics Conference Proceedings: How Trees Stand Up and Fall Down (eds. Smiley E.T. & Coder K.D.) 53–70 (Champaign, Illinois: International Society of Arboriculture, 2001). http://www.tree-consult.org/upload/mediapool/pdf/baumstatik_und_biomechanik/trees-and-statics-nondestructive-failure-analysis.pdf Accessed 2 May 2019.
13. Detter, A. & Rust, S. Aktuelle Untersuchungsergebnisse zu Zugversuchen. pp. 87-100. In: D.

Dujesiefken (Ed.). *Jahrbuch der Baumpflege* (Haymarket Media, 2013). (ISBN 978-3-87815-243-9)

14. Göcke, L., Rust, S. & Ruhl F. Assessing the anchorage and critical wind speed of urban trees using root-plate inclination in high winds. *Arboriculture & Urban Forestry* **44(1),** 1-11 (2018).
15. Vojáčková, B. et al. The effect of stem and root-plate defects on the tree response during static loading—Numerical analysis. *Urban Forestry & Urban Greening* **59,** 127002 https://doi.org/10.1016/j.ufug.2021.127002.2021 (2021).
16. Mattheck, C. & Breloer, H. *The body language of trees: A handbook for failure analysis. (*London, TSO, 1997). (ISBN 978-0117530676)
17. Mattheck, C., Betghe, K. & Erb, D. Versagenskriterien für Baume. *Allg. Forst-u. J.-Ztg.* **164(1),** 9–12 (1992).
18. Göcke, L. & Rust, S. Correlation of wind speed and root plate tilt of trees in urban environment. Presented at the ISA annual conference and trade show, 8th August 2015, Orlando, FL, USA. Accessed on January 7, 2022. https://www.researchgate.net/publication/281204270_Correlation_of_wind_speed_and_root_plate_tilt_of_trees_in_urban_environment (2015).
19. MyCurveFit, Data Assays Ltd. https://mycurvefit.com/ Accessed 28 November 2021.
20. Peltola, H.M. Mechanical stability of trees under static loads. *Am. J. Bot.* **93,** 1501-1511 https://doi.org/10.3732/ajb.93.10.1501 (2006).
21. Pantaleo, A., Pellerano, A., Ferrini, F. & Villa, G. Modelling the stability of Phoenix canariensis palms under external loads: An application to a real case. *Adv. Hort. Sci.* **19(2),** 101–110 (2005).
22. Not applicable.
23. Wilczynski, S. M. 2017 *Critical Specialties-Treating Autism & Behavioral Challenge, A Practical Guide to Finding Treatments That Work for People with Autism.* (London: Academic Press).
24. Ioannidis, J.P.A. 2005 Why most published research findings are false. *PLoS Med* **2,** 8: e124 https://doi.org/10.1371/journal.pmed.0020124
25. Griffith, A. V. I. The phenomena of rupture and fow in solids. *Proc. R. Soc.* **221,** 582–593. https://doi.org/10.1098/rsta.1921.0006 (1921).
26. Not applicable.
27. Virot, E., Ponomarenko, A., Dehandschoewercker, E., Quéré, D. & Clanet, C. Critical wind speed at which trees break. *Phys. Rev. E* **93,** 023001. https://doi.org/10.1103/PhysRevE.93.023001 (2016).
28. Not applicable.
29. Not applicable..
30. Vollsinger, S., Mitchell, S. J., Byrne, K. E., Novak, M. D., & Rudnicki, M. Wind tunnel measurements of crown streamlining and drag relationships for several hardwood species. *Canadian Journal of Forest Research* **35(5),** 1238–1249. https://doi.org/10.1139/X05-051 (2005).
31. Zeng, F., Lei, C., Liu, J., Niu, J. & Gao, N. CFD simulation of the drag effect of urban trees: Source term modification method revisited at the tree scale. *Sustainable Cities and Society* 56, 102079 https://doi.org/10.1016/j.scs.2020.102079 (2020).
32. Albrecht, A., Badel, E., Bonnesoeur, V., Brunet, Y., Constant, T., Défossez, P., de Langre, E., Dupont, S., Fournier, M., Gardiner, B., J. Mitchell, S., Moore, J.-R-, Moulia, B., Nicoll, B.C., Niklas, K.-J., Schelhaas, M.-J., Spatz, H.-C. & Telewski F.W. Comment on "Critical wind speed at which trees break". *Physical Review* E **94,** 067001 https://link.aps.org/doi/10.1103/PhysRevE.94.067001 (2016).
33. Gardiner, B., Achim, A., Nicoll, B., & Ruel, J.-C. Understanding the interactions between wind and trees: An introduction to the IUFRO 8th Wind and Trees Conference (2017).

Forestry: An International Journal of Forest Research **92(4),** 375–380. https://doi.org/10.1093/forestry/cpz044 (2019).

34. Gardiner, B., Byrne, K., Hale, S., Kamimura, K., Mitchell, S.J., Peltola, H. & Ruel, J.-C. A review of mechanistic modelling of wind damage risk to forests, *Forestry: An International Journal of Forest Research* **81(3),** 447–463. https://doi.org/10.1093/forestry/cpn022 (2008).
35. Not applicable.
36. Not applicable.
37. Detter, A., Rust, S., Bötcher, S. & Bouillon, E. Ambient influences on the results of non-destructive pulling tests. pp. 382-386. In: Eds. Xiping, W., Udo H.S, & Robert J.R. *Proceedings: 21st International Nondestructive Testing and Evaluation of Wood Symposium*. General Technical Report FPL-GTR-272. Madison, WI: U.S. (Department of Agriculture, Forest Service, Forest Products Laboratory, 2019).
38. Rust, S. & Detter, A. Experimental Test of Nondestructive Methods to Assess the Anchorage of Urban Trees. pp. 417-421. In: Eds. Xiping, W., Udo H.S, & Robert J.R. *Proceedings: 21st International Nondestructive Testing and Evaluation of Wood Symposium*. General Technical Report FPL-GTR-272. Madison, WI: U.S. (Department of Agriculture, Forest Service, Forest Products Laboratory, 2019).
39. Dellus, V. Wessolly, L., Erb, M. & Detter A. Tree safety evaluation by pulling test and wind load analysis. In: *7th Proceeding of the Plant Biomechanics International Conference*. (Eds. Moulia, B. & M. Fournier. 7th Plant Biomechanics International Conference, Aug 2012, Clermont-Fd, France. INRA, 394 p., 2012, 2105-1089. Accessed January 1, 2022. https://hal.archives-ouvertes.fr/hal-01195122
40. Buza, Á. K. & Divós, F. Root stability evaluation with non-destructive techniques. *Acta Silvatica et Lignaria Hungarica* **12,** 125–134 https://dx.doi.org/10.1515/aslh-2016-0011 (2016).
41. Rinn, F. Principles and challenges of static load tests ('pull-testing') for estimating uprooting safety. *Western Arborist.* **1,** 36-41 (2017).
42. Jackson, T. D., Sethi, S., Dellwik, E., Angelou, N., Bunce, A., van Emmerik, T., Duperat, M., Ruel, J.-C., Wellpott, A., Van Bloem, S., Achim, A., Kane, B., Ciruzzi, D. M., Loheide II, S. P., James, K., Burcham, D., Moore, J., Schindler, D., Kolbe, S., Wiegmann, K., Rudnicki, M., Lieffers, V. J., Selker, J., Gougherty, A. V., Newson, T., Koeser, A., Miesbauer, J., Samelson, R., Wagner, J., Ambrose, A. R., Detter, A., Rust, S., Coomes, D., & Gardiner, B. The motion of trees in the wind: a data synthesis, *Biogeosciences* **18,** 4059–4072, https://doi.org/10.5194/bg-18-4059-2021 (2021).
43. James, K. R., Haritos, N., & Ades, P. K. Mechanical stability of trees under dynamic loads. *American Journal of Botany* **93(10),** 1522–1530. (2006).
44. van Emmerik, T., Steele-Dunne, S., Hut, R., Gentine, P., Guerin, M., Oliveira, R.S., Wagner, J., Selker, J., & Van de Giesen, N. Measuring Tree Properties and Responses Using Low-Cost Accelerometers. MPDI. *Sensors* **17(5),** 1098. https://doi.org/10.3390/s17051098 (2017).
45. Oviedo-Garcıa, M. A. Journal citation reports and the definition of a predatory journal: The case of the Multidisciplinary Digital Publishing Institute (MDPI). *Research Evaluation* **30(3),** 405–419 https://doi.org/10.1093/reseval/rvab020 (2021).
46. Rust, S., Göcke, L. & Detter, A. Wind Tipping Curves – Assessing the Anchorage and Wind Load of Urban Trees by Motion Monitoring. *Proceedings of the 21st non-destructive testing and evaluation of wood symposium* (pp 1-5). (Germany, Forest Research Institute Baden-Württemberg). Retrieved from the Researchgate page of the first author on 28 December 2019 (2019).
47. Jackson, T., et al. 2019 An architectural understanding of natural sway frequencies in trees. *J. R. Soc. Interface* **16,** 20190116. http://dx.doi.org/10.1098/rsif.2019.011 (2019).
48. Sagi, P. Newson, T., Miller, C., & Mitchell, S. Stem and root system response of a Norway

spruce tree (*Picea abies* L.) under static loading. *Forestry: An International Journal of Forest Research* **92(4),** 460–472. https://doi.org/10.1093/forestry/cpz042 (2019).
49. Steneck, N.H. Fostering integrity in research: Definitions, current knowledge, and future directions. *Science and Engineering Ethics* **12,** 53–74 https://doi.org/10.1007/PL00022268 (2006).
50. Jackson, T.D., Shenkin, A.F., Majalap, N., et al. The mechanical stability of the world's tallest broadleaf trees. *Biotropica* **53,** 110–120. https://doi.org/10.1111/btp.12850 (2021).
51. Schindler, D., Vogt, R., Fugmann, H., Rodriguez, M., Schönborn, J. & Mayer, H. Vibration behavior of plantation-grown Scots pine trees in response to wind excitation. *Agricultural and Forest Meteorology* **150,** 7–8: 984-993. https://doi.org/10.1016/j.agrformet.2010.03.003 (2010).
52. Schindler, D. & Mohr, M. Non-oscillatory response to wind loading dominates movement of Scots pine trees. *Agricultural and Forest Meteorology* **250–251,** 209-216. https://doi.org/10.1016/j.agrformet.2017.12.258 (2018).
53. Giambastiani, Y., Preti, F. Errico, A. & Sani, L. On the tree stability: pulling tests and modelling to assess the root anchorage. *Procedia Environmental Science, Engineering and Management* **4,** 4: 207-218 (2017).
54. Schindler, D. & Kolbe, S. Assessment of the Response of a Scots Pine Tree to Effective Wind Loading. MPDI.. *Forests* **11,** 2: 145. https://doi.org/10.3390/f11020145 (2020).
55. Luca, M., Grigolato, S., Mologni, O., Scotta, R., Cavalli, R. & L. Montecchio. State of the Art on the Use of Trees as Supports and Anchors in Forest Operations. MPDI. *Forests* **9,** 8: 467. https://doi.org/10.3390/f9080467 (2018).
56. Krisans, O., Matisons, R., Rust, S., Burnevica, N., Bruna, L., Elferts, D., Kalvane, L. & Jansons, A. "Presence of Root Rot Reduces Stability of Norway Spruce (*Picea abies*): Results of Static Pulling Tests in Latvia". MPDI. *Forests* **11,4:** 416. https://doi.org/10.3390/f11040416 (2020).
57. Linhares, C.S.F., Gonçalves, R., Martins, L.M. & Knapic, S. Structural Stability of Urban Trees Using Visual and Instrumental Techniques: A Review. MPDI. *Forests* **12,** 1752. https://doi.org/10.3390/f12121752 (2021).
58. Moravacík, L'., Vincúr, R. & Rózová, Z. Analysis of the StaticBehavior of a Single Tree on a FiniteElement Model. MPDI. *Plants* **10,** 1284. https://doi.org/10.3390/plants10071284 (2021).
59. Mattheck, C. *Stupsi explains the tree: A hedgehog teaches the body language of trees.* (Karlsruher Institut für Technologie, 1999). (ISBN 10-3923704224)
60. Forbes-Laird, J. Evidence (Arboriculture): Matter: Mullinger & Others v The National Trust. Claim No. HQ10X1869. Instruction ref. RC27-1061. Report. (UK, Forbes-Laird Arboricultural Consultancy, April 2011). https://www.qtra.co.uk/cms/index.php?action=download&id=205&module=downloadmodule&src=%40random4ecbc721b3d91 Accessed 12 July 2021.
61. Green, B.M. *An estimation of critical wind speed and a calculation of mechanical properties for two Australian eucalypts* (Thesis). School of Aerospace Mechanical and Manufacturing Engineering. College of Science Engineering and Health. RMIT University, Australia (2016).
62. Baker, M. 1,500 scientists lift the lid on reproducibility. *Nature*, ***533***, 452-454 (2016).
63. Mattheck, C & Bethge, K. Abridged response to the criticism of VTA-failure criteria by Franz Gruber. *Arboricultural Journal* **31(4),** 273-276 https://doi.org/10.1080/03071375.2008.9747514 (2008).
64. Beall, J. Beall's List of Potential Predatory Journals and Publishers. https://beallslist.net/#update. Accessed 6 June 2021.
65. Anderson, R. Guest Post—Why Should We Worry about Predatory Journals? Here's One Reason. https://blog.cabells.com/2020/03/03/guest-post-why-should-we-worry-about-

predatory-journals-heres-one-reason/ Accessed 6 June 2021 (2020).
66. Dargahi, M., Newson, T., and R. Moore, J. A numerical approach to estimate natural frequency of trees with variable properties, *Forests,* 11, 915, https://doi.org/10.3390/f11090915 (2020).
67. Drénou, C., Restrepo, D. & Slater, D. Demystifying Tree Forks: Vices and Virtues of Forks in Arboriculture. MPDI. *J Bot Res,* **3(1),** 100-113. http://dx.doi.org/10.36959/771/566 (2020).
68. Xu, P., Guan, C., Zhang, H., Li, G., Zhao, D., Ross, R.J., & Shen, Y. Application of Nondestructive Testing Technologies in Preserving Historic Trees and Ancient Timber Structures in China. MPDI. *Forests* **12,** 318. https://doi.org/10.3390/f12030318 (2021).
69. Allison, R.B., Xiping W. & Christopher A.S. Methods for Nondestructive Testing of Urban Trees. *Forests* **11,** 12: 1341. https://doi.org/10.3390/f11121341 (2020).
70. Anyomi, K.A., Mitchell, S.J., Perera, A.H. & Ruel, J.-C. Windthrow Dynamics in Boreal Ontario: A Simulation of the Vulnerability of Several Stand Types across a Range of Wind Speeds. *Forests* **8,** 233. https://doi.org/10.3390/f8070233 (2017).
71. Kolbe, S., Mohr, M., Maier, M., Osterholt, L., Gardiner, B. & Schindler, D. On the Potential of Using Air Pressure Fluctuations to Estimate Wind-Induced Tree Motion in a Planted Scots Pine Forest. *Forests* **13,** 225. https://doi.org/10.3390/f13020225 (2022).
72. Fathi, S., Bejo, L., & Divos, F. Investigating the Effect of Weather and Seasonal Factors on Root Stability Using Dynamic Measurements. SCIRP. *Open Journal of Forestry* **10,** 124-134. https://doi.org/10.4236/ojf.2020.101009 (2020).
73. Murray, R. Skillful writing of an awful research paper. *Anal. Chem.* **83,** 3: 633 https://doi.org/10.1021/ac2000169 (2011).