Dear Dr. Armin Sorooshianlaw and Natascha Töpfer,

Your e-mail of 19 May recognizes that I replied fairly early in the discussion period to all of the interactive comments, consistent with the *Atmospheric Chemistry and Physics* objective to stimulate further discussion by interested scientists. However, that e-mail also specified that "the response to the Referees shall be structured in a clear and easy to follow sequence: (1) comments from Referees, (2) author's response, (3) author's changes in manuscript". Therefore, I include here copies of my replies, with a format that orders comments from Referees (in normal font), **author's response (in bold)**, and *author's changes in manuscript (in italics)*.

Please note that the replies below are also slightly different from the on-line replies that I posted in April, and the manuscript has also changed slightly as I have had more time to improve it. All of these changes are noted below using track changes in Word, for the convenience of the editor and reviewers.

I look forward to hearing from you regarding the possibility that this manuscript can be published in *Atmospheric Chemistry and Physics*.

Sincerely,

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Changes Not Caused by Referee Comments

The author has discovered one substantial and two minor issues with the manuscript that he believes require modifications, independent of the comments from these three referees.

The substantial issue regards a comment from the original submission, prior to publication on-line in *Atmospheric Chemistry and Physics Discussions* (comment number 6 from the 2nd reviewer; March, 2017), and regards the nomenclature describing eqs. (1) and (3). The reviewer's comment was very insightful, but both the author's reply and original modification to the manuscript were inappropriate.

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The author's reply was:
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1. *Because the text in the 4th paragraph of the introduction is truly supportive of the 3rd paragraph of the introduction, the author has removed the paragraph break and merged these two paragraphs into one.*
2. The author has deleted the repeated word "is" in the second line of the Conclusions section.

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> The representative evaporation rate prescribed in Table 3 and vertical velocity resulting from eq. (4) are valid for most of the scales defined above. Thus, the boundary condition $w|_0$ is valid for the synoptic scale (notwithstanding vertical motion aloft, such as subsidence), for the micrometeorological scale, and even for the leaf scale. In the context of scale analysis, leaves may be approximated as having equal area as the underlying surface (i.e., a unit leaf area index, or LAI=1), and equal evaporation rates as the surface in general. This latter assumption does not neglect soil evaporation, but only excludes the possibility that it dominate leaf evaporation by an order of magnitude. Thus, it will be assumed here that the assumed evaporation rate and derived vertical velocities are equally valid at synoptic (A), micrometeorological (B), and leaf (C) scales. The order of magnitude is different, however, at the microscopic (D) scale. To show this, it will be assumed here that all leaf evaporation (or transpiration) occurs through the small fraction of the leaf that is stomatal (σ), such that both the stomatal evaporative flux density and the lower boundary condition for the vertical velocity ($w|_0$) are a factor $1/\sigma$ greater than that at larger scales. Independent of scale, Eq. (4) states that, for a positive evaporation rate, the boundary condition for the vertical velocity is non-zero and upward.

Line 404. 'average air speed exiting a stomatal aperture is 3.1 mm s-1.' I would find interesting if the author could provide a plot or a table showing how the main physical (pore size) or environmental variables (T? P?) affect this velocity. The roles of such variables in modulating this velocity are quite small in comparison with its near-direct, linear dependence on the evaporation rate, as described by eq. (4). Such dependences do not, in the author's opinion, merit depiction via a plot or a table. *The author has made no changes as a result of this comment.*

Line 436. 'described in many chemical engineering texts'. Any references? The author agrees. *The author has added references to three chemical engineering texts here (Kreith et al., 1999; Lienhard and Lienhard, 2000; Bird et al., 2002).*

Line 471: Any more recent references about helox experiments? The author is unaware of more recent references regarding helox experiments, but points out that the Mott and Parkhurst (1991) paper has been cited more than 300 times (Web of Science). *The author has made no changes as a result of this comment.*

Line 483: import->importance(?). *The author has made this proposed change.*

Line 477 and following (Conclusions).I suggest to remove also from here uncommon symbols or to explain them all. More generally, I would still have a question: do the non-diffusive process described in the text have computational or only theoretical/descriptive effects?
The author agreed to explain the symbols in the conclusions section. However, upon revising the conclusions, the author finds that the "uncommon" symbols are already largely explained. The sentence in question states that "the boundary condition for the vertical velocity is $w|_0 = \frac{E}{\rho|_0}$, where $\rho|_0$ is the air density at the surface". *As a result of this comment, the author has added at line 510 the adjective "lower"* such that this sentence now reads "the lower boundary condition for the vertical velocity is $w|_0 = \frac{E}{\rho|_0}$, where $\rho|_0$ is the air density at the surface". In this way, both of the "uncommon" symbols are explained.

4

The computation of non-diffusive transport is given by equation (7). This is the amount of transport that should be subtracted from total transport in order to characterize diffusive transport, which is the quantity that is relevant to the derivation of flux-gradient relationships such as the eddy diffusivity and/or stomatal conductance. The purpose of the last three paragraphs of section 3.3 is to compute, for particular gases using representative flux magnitudes and environmental conditions, the magnitudes of these "corrections" to the eddy diffusivity and/or stomatal conductance. *The author has made no changes as a result of this comment about computational/theoretical effects.*

In Figure 2, it could be helpful if the presence of Mercury, and the circumstance that the tube is open to the atmosphere, would be indicated in the design. The author agrees. *The author has modified Figure 2, inserting the information regarding the mercury in the manometer and the fact that the tube is open to the atmosphere.*

Replies to Referee #2 (W. Eugster)

The author is known for his accurate and meticulous assessment of very fundamental aspects of atmospheric physics. In his present paper he addresses an issue that has led to many discussions before and which has not been convincingly solved so far: the magnitude of the vertical motion in the planetary boundary layer near the Earth's surface, a motion that is too small to be accurately measured with present-day state-of-the-art ultrasonic anemometers, but which is still large enough to affect (eddy covariance) flux measurements of trace gases.

So far most scientists would agree that at a certain small height above the solid ground surface, the roughness height $z\big|_0$ (in Kowalski's notation) the mean horizontal wind speed must be 0 ms^{-1}, and also mean vertical wind speed $w\big|_0$ should be 0 ms^{-1}, a boundary condition that Kowalski questions on good grounds. He links $w\big|_0$ directly to the moisture flux density (E). He develops his theory based on the one-dimensional equation

$$w\rho = \sum_{i=1}^{N} w_i \rho_i. \tag{1}$$

with w_i and ρ_i being the vertical velocity and partial density of gas component *i* in a gas mixture with *N* components. Conceptually this is a hydrostatic approach that only allows for expansion in the vertical direction, which may exaggerate the magnitude of vertical velocity w. Hence, in Section 2.3 Kowalski expands to the full 3-d advectiondiffusion concept that should better represent reality.

The author thanks Dr. Eugster for this assessment, which shows that the manuscript has managed to communicate the essence of the theory being developed. *The author has made no changes as a result of this comment.*

My main critique – although I must admit that my own understanding of atmospheric physics is not nearly up to the level of that of Kowalski's – is the following:

1. Kowalski primarily associates the vertical velocity at roughness height $z\big|_0$ with the evaporative flux E but not with the vertical sensible heat flux H. I would assume that this is only correct for H = 0 W m^{-2}, but not for any other magnitude of sensible heat flux. In my view a partial gas density expressed as ρ_i in kg m^{-3} has it's volume component affected by both sensible and latent heat fluxes – and all other gas component fluxes (which however can be neglected, I agree on this aspect). An explicit treatment of the effect of H would be essential in my view to help the average reader (like myself) better understand the concept and argumentation.

The author disagrees here, and points to his previous publications that address this very issue.

Certainly, it is traditional in micrometeorology to infer a mean velocity in the direction of the sensible heat flux (Webb et al., 1980; "WPL"), as Dr. Eugster asserts. However, Kowalski (2012) showed that this inference is an artefact of imprecise averaging procedures. Perhaps the simplest illustration of the WPL error is given by the scenario visualized in the artless drawing in Figure X below, where turbulent air convection is enclosed within a stationary chamber with an upward heat flux under steady-state conditions.
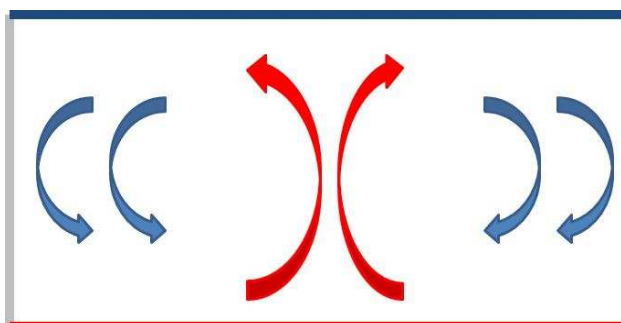
Figure X. A chamber with a heated floor (red), chilled ceiling (blue), and insulated side walls (grey). The inside air is in steady state and turbulent (convective) due to an unstable lapse rate.

The most fundamental definition of the mean velocity of a system is the ratio of its displacement to the time elapsed. For steady-state air confined to a stationary chamber, its displacement over long time periods is clearly zero, and therefore so is the average velocity, notwithstanding WPL predictions of an upward velocity associated with the upward heat flux.

For non-steady-state conditions, average velocities can occur due to expansion/compression of the air layer near a surface boundary. For example, in Figure X, assuming that temperature effects dominate those of pressure in determining air density, there is more air near the cool ceiling than at the hot floor. Thus, eliminating floor heating and ceiling cooling would result in a net downward transport of air. Such effects are treated by Kowalski and Serrano-Ortiz (2008) for a simple scenario, neglecting evaporation's influence on the average velocity, and demonstrating conceptual errors in the WPL velocity. A more general derivation, combining the effects of compressibility and vapor exchanges, is beyond the scope of the current discussion.

The author has made no changes as a result of this comment.

2. In principle the concept and analysis could be expanded to the different isotopes (stable or unstable, but the treatment of unstable isotopes would probably add yet another layer of complexity) of each gas component. At least the coverage of stable isotopes might be helpful in context with "counter-gradient isotope fluxes" that tend to be brought up occasionally.

The author agrees that the concept and analysis could be expanded to the issue of isotope transport, but is ignorant of the phenomenon of "counter-gradient isotope fluxes". Perhaps Dr. Eugster could provide references for these observations (?). Otherwise, it seems that this issue is beyond the scope of the present manuscript, but could be worthy of future investigation.

The author has made no changes as a result of this comment, but remains interested in learning more about "counter-gradient isotope fluxes".

3. It would be appreciated to reword some passages where plant physiologists and plant ecologists are non-neutrally qualified as partially ignorant scientists. I must admit that I had a private discussion with Graham Farquhar at a conference in Interlaken more than 10 years ago about "counter-gradient isotope fluxes" and actually had the feeling that it is fruitful in interdisciplinary work to exchange ideas between disciplines, but should not consider ourselves superior to those who start to dig into new terrain (from their perspective) – we tend to leave a similarly bleak trace if we dare to lean outside of our own territory. I think it is the strength of interdisciplinary researchers that they take the risk to be considered a non-savant outside their area of profound expertise, and we should restrain from spreading bad marks to others from other disciplines (this relates mostly to lines 394–395, 410, 415–424).

The author is aware that some readers find his writing style to be offensive, has worked hard to try to correct this problem, and is not surprised to find that it persists. There can be little doubt regarding the benefits of exchanging ideas between disciplines, and any specific recommendations would be very welcome regarding how to reword passages so as to be more neutral.

However, having said that, the author is unwilling to refrain from criticizing theories or procedures that are incorrect. The objective here is not to consider anyone as superior, but rather to discover and defend the truth, and the author has taken great care to do this based solidly on fundamental physical laws. From the author's point of view, Jarman (1974) neglected momentum conservation, erroneously classified the description of Parkinson and Penman (1970) as "incorrect" (twice) or "substantially incorrect" (twice more), and misled an entire community of scientists along a mistaken path for several decades. The key question then is how correct this error and prevent its further propagation.

In an attempt to comply with Dr. Eugster's suggestion and avoid defaming a particular discipline, the author has adopted the following changes

- *Lines 419: delete ",as has been neglected by the discipline of plant physiology, or ecophysiology";*
- *Line 434: delete ", but broadly neglected in the field of ecophysiology"*
- *Line 447: delete "among plant physiologists"*

4. The conclusions end with a very general take-home message, but since the author puts so much emphasis in his text to educate plant ecologists, it would be beneficial to have a more specific recommendation set for what plant ecologists finally are supposed to do with this new-gained knowledge. This does not explicitly become clear and the paper would benefit by having such explicit, specific recommendations that I and other could easily pik up, understand, and implement in our own calculations.

Unfortunately, the author has not been able to develop a "quick fix": an algorithm or equation that would immediately correct WUE or c_i by accounting for non-diffusive transport. This therefore falls under the category of future research, and is open to any scientist who may have better ideas about solving these tricky issues. *The author has made no changes as a result of this comment.*

"Minor technical issues":
L. 86: add "vertical" before velocity

The author has made this change (now line 89).

L. 403: this appears to be the old notation of the previous (internal) version and should now read (w$\vert_0$)

The author has made this change (now line 427). The author found and corrected the same problem in the last paragraph of the discussion (now line 505).

Replies to Anonymous Referee #3

General comments: Vertical velocity has a tiny magnitude near surface and is difficult to measure because its magnitude is usually smaller than errors. However, vertical velocity plays a substantial role in mass and energy exchanges between land and atmosphere. For simplicity, they usually assume it is zero at surface. The author argues that it is non-zero by a "thought experiment". The author is a theoretical thinker. This paper shines light on this knowledge gap. I recommend it to be published with minor revision.

The author thanks the referee for this endorsement. The *author has made no changes as a result of this comment.*

Specific comments:

(1) 2.1.2 The 0th Law of Thermodynamics – I do believe that this is a case from second law of thermodynamics (Postulate of Clausius, see Thermodynamics by Enrico Fermi, 1936). I don't think that "The 0th Law of Thermodynamics" is independent from second law of thermodynamics. So I suggest using the second law of thermodynamics instead of the 0th Law so that your statements no matter heat transfer and mass diffusion are govern by the same second law of thermodynamics. Fourier's law and Fick's law are empirical relationships between fluxes and gradients. Gradients are drivers for fluxes and consequences of fluxes reduce gradients, following a single irreversible direction (entropy increasing) – equilibrium (entropy maximum) –second law of thermodynamics.

The 1936 textbook cited, although authored by a great physicist, is nonetheless out of date regarding this issue. Modern physics texts (e.g., Giancoli, 1984) recognize that the 1st and 2nd Laws, although definitively stated first, logically depend on the prior assertion of the 0th Law; this explains its odd name. Formalization of the 0th Law occurred in the mid-1930s, but was not broadly accepted until well after the publication of Fermi's textbook. The *author has made no changes as a result of this comment.*

(2) Vertical velocity at surface is always positive (upward) predicted by the equation (4). Based on your thought experiment, this looks true everywhere (leaves, ground, water surface) including large scale (e.g. synoptic scale). To my knowledge, it is sure that vertical velocity is negative in high pressure system areas and positive in low pressure system areas. Therefore, it is difficult for me to understand the positive vertical velocity predicted by your theory in high pressure system areas or divergent air-flow near surface at any scale. Please clarify the conflict in your revision.

Synoptic-scale velocities are of order 3 cm s^{-1} (e.g., Carlson and Stull, 1986, Subsidence in the nocturnal boundary layer, *J. Clim. Appl. Met.*, **25**, 1088-1099). Whether the boundary condition at the surface is the traditionally conceived $\mathbf{w}\big|_0 = 0$ cm s^{-1}, or 0.000031 cm s^{-1} as derived from eq. (4), synoptic-scale subsidence implies a convergence in the vertical winds between the surface and the height at which it occurs. It seems that there is no conflict that requires clarification. The *author has made no changes as a result of this comment.*

(3) Page 6 second paragraph, It is fine to me with "vertical advection" because it is clearly defined by vertical component It does not need to assume horizontal homogeneity.

The author agrees, and initially proposed simply to delete the last two sentences of this paragraph. However, upon careful reconsideration, the author wishes to finish this paragraph with a statement to clarify a key distinction between advection and diffusion. *Therefore, the author has deleted the two sentences at the end of the first paragraph of section 2.3 (and consequentially the Rannik et al., 2009 paper has been removed from the references section), and added a new sentence to finish this paragraph (now at lines 213-214), to say that:* "Thus advection, unlike diffusion, is not a form of transport, but rather a consequence of differential transport."

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To make this somewhat more clear to the reader, the author has modified the parenthetical remark – now at line 200 - to say '(e.g., "at a point", in an Eulerian fluid specification)'.

Line 223 and following. Are these four cases, all similar, strictly necessary? A single case study of the size of a leaf (e.g., 1 cm2) would simplify the text. The author feels that the four cases are indeed worth considering, for the simple reason that the derived velocities represent spatial averages that are valid over a variety of scales, the larger of which are of particular interest to meteorologists. However, the manuscript neglected to make this explicit. *As a result, the author has changed the paragraph that begins at 299 to the following:*

> The representative evaporation rate prescribed in Table 3 and vertical velocity resulting from eq. (4) are valid for most of the scales defined above. Thus, the boundary condition $w\big|_0$ is valid for the synoptic scale (notwithstanding vertical motion aloft, such as subsidence), for the micrometeorological scale, and even for the leaf scale. In the context of scale analysis, leaves may be approximated as having equal area as the underlying surface (i.e., a unit leaf area index, or LAI=1), and equal evaporation rates as the surface in general. This latter assumption does not neglect soil evaporation, but only excludes the possibility that it dominate leaf evaporation by an order of magnitude. Thus, it will be assumed here that the assumed evaporation rate and derived vertical velocities are equally valid at synoptic (A), micrometeorological (B), and leaf (C) scales. The order of magnitude is different, however, at the microscopic (D) scale. To show this, it will be assumed here that all leaf evaporation (or transpiration) occurs through the small fraction of the leaf that is stomatal (σ), such that both the stomatal evaporative flux density and the lower boundary condition for the vertical velocity ($w\big|_0$) are a factor $1/\sigma$ greater than that at larger scales. Independent of scale, Eq. (4) states that, for a positive evaporation rate, the boundary condition for the vertical velocity is non-zero and upward.

Line 404. 'average air speed exiting a stomatal aperture is 3.1 mm s-1.' I would find interesting if the author could provide a plot or a table showing how the main physical (pore size) or environmental variables (T? P?) affect this velocity. The roles of such variables in modulating this velocity are quite small in comparison with its near-direct, linear dependence on the evaporation rate, as described by eq. (4). Such dependences do not, in the author's opinion, merit depiction via a plot or a table. *The author has made no changes as a result of this comment.*

Line 436. 'described in many chemical engineering texts'. Any references? The author agrees. *The author has added references to three chemical engineering texts here (Kreith et al., 1999; Lienhard and Lienhard, 2000; Bird et al., 2002).*

Line 471: Any more recent references about helox experiments? The author is unaware of more recent references regarding helox experiments, but points out that the Mott and Parkhurst (1991) paper has been cited more than 300 times (Web of Science). *The author has made no changes as a result of this comment.*

Line 483: import->importance(?). *The author has made this proposed change.*

Line 477 and following (Conclusions).I suggest to remove also from here uncommon symbols or to explain them all. More generally, I would still have a question: do the non-diffusive process described in the text have computational or only theoretical/descriptive effects?
The author agreed to explain the symbols in the conclusions section. However, upon revising the conclusions, the author finds that the "uncommon" symbols are already largely explained. The sentence in question states that "the boundary condition for the vertical velocity is $w\big|_0 = \frac{E}{\rho\big|_0}$, where $\rho\big|_0$ is the air density at the surface". *As a result of this comment, the author has added at line 510 the adjective "lower" such that this sentence now reads* "the lower boundary condition for the vertical velocity is $w\big|_0 = \frac{E}{\rho\big|_0}$, where $\rho\big|_0$ is the air density at the surface". In this way, both of the "uncommon" symbols are explained.

Con formato: Fuente: Sin Cursiva

4

The computation of non-diffusive transport is given by equation (7). This is the amount of transport that should be subtracted from total transport in order to characterize diffusive transport, which is the quantity that is relevant to the derivation of flux-gradient relationships such as the eddy diffusivity and/or stomatal conductance. The purpose of the last three paragraphs of section 3.3 is to compute, for particular gases using representative flux magnitudes and environmental conditions, the magnitudes of these "corrections" to the eddy diffusivity and/or stomatal conductance. *The author has made no changes as a result of this comment about computational/theoretical effects.*

In Figure 2, it could be helpful if the presence of Mercury, and the circumstance that the tube is open to the atmosphere, would be indicated in the design. The author agrees. *The author has modified Figure 2, inserting the information regarding the mercury in the manometer and the fact that the tube is open to the atmosphere.*

Replies to Referee #2 (W. Eugster)

The author is known for his accurate and meticulous assessment of very fundamental aspects of atmospheric physics. In his present paper he addresses an issue that has led to many discussions before and which has not been convincingly solved so far: the magnitude of the vertical motion in the planetary boundary layer near the Earth's surface, a motion that is too small to be accurately measured with present-day state-of-the-art ultrasonic anemometers, but which is still large enough to affect (eddy covariance) flux measurements of trace gases.

So far most scientists would agree that at a certain small height above the solid ground surface, the roughness height $z\rfloor_0$ (in Kowalski's notation) the mean horizontal wind speed must be 0 ms^{-1}, and also mean vertical wind speed $w\rfloor_0$ should be 0 ms^{-1}, a boundary condition that Kowalski questions on good grounds. He links $w\rfloor_0$ directly to the moisture flux density (E). He develops his theory based on the one-dimensional equation

$$w\rho = \sum_{i=1}^{N} w_i \rho_i. \tag{1}$$

with w_i and ρ_i being the vertical velocity and partial density of gas component *i* in a gas mixture with *N* components. Conceptually this is a hydrostatic approach that only allows for expansion in the vertical direction, which may exaggerate the magnitude of vertical velocity w. Hence, in Section 2.3 Kowalski expands to the full 3-d advectiondiffusion concept that should better represent reality.

The author thanks Dr. Eugster for this assessment, which shows that the manuscript has managed to communicate the essence of the theory being developed. *The author has made no changes as a result of this comment.*

My main critique – although I must admit that my own understanding of atmospheric physics is not nearly up to the level of that of Kowalski's – is the following:

1. Kowalski primarily associates the vertical velocity at roughness height $z\rfloor_0$ with the evaporative flux E but not with the vertical sensible heat flux H. I would assume that this is only correct for H = 0 W m^{-2}, but not for any other magnitude of sensible heat flux. In my view a partial gas density expressed as ρ_i in kg m^{-3} has it's volume component affected by both sensible and latent heat fluxes – and all other gas component fluxes (which however can be neglected, I agree on this aspect). An explicit treatment of the effect of H would be essential in my view to help the average reader (like myself) better understand the concept and argumentation.

The author disagrees here, and points to his previous publications that address this very issue.

Certainly, it is traditional in micrometeorology to infer a mean velocity in the direction of the sensible heat flux (Webb et al., 1980; "WPL"), as Dr. Eugster asserts. However, Kowalski (2012) showed that this inference is an artefact of imprecise averaging procedures. Perhaps the simplest illustration of the WPL error is given by the scenario visualized in the artless drawing in Figure X below, where turbulent air convection is enclosed within a stationary chamber with an upward heat flux under steady-state conditions.
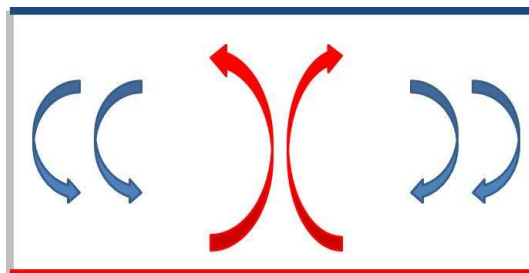
Figure X. A chamber with a heated floor (red), chilled ceiling (blue), and insulated side walls (grey). The inside air is in steady state and turbulent (convective) due to an unstable lapse rate.

The most fundamental definition of the mean velocity of a system is the ratio of its displacement to the time elapsed. For steady-state air confined to a stationary chamber, its displacement over long time periods is clearly zero, and therefore so is the average velocity, notwithstanding WPL predictions of an upward velocity associated with the upward heat flux.

For non-steady-state conditions, average velocities can occur due to expansion/compression of the air layer near a surface boundary. For example, in Figure X, assuming that temperature effects dominate those of pressure in determining air density, there is more air near the cool ceiling than at the hot floor. Thus, eliminating floor heating and ceiling cooling would result in a net downward transport of air. Such effects are treated by Kowalski and Serrano-Ortiz (2008) for a simple scenario, neglecting evaporation's influence on the average velocity, and demonstrating conceptual errors in the WPL velocity. A more general derivation, combining the effects of compressibility and vapor exchanges, is beyond the scope of the current discussion.

The author has made no changes as a result of this comment.

2. In principle the concept and analysis could be expanded to the different isotopes (stable or unstable, but the treatment of unstable isotopes would probably add yet another layer of complexity) of each gas component. At least the coverage of stable isotopes might be helpful in context with "counter-gradient isotope fluxes" that tend to be brought up occasionally.

The author agrees that the concept and analysis could be expanded to the issue of isotope transport, but is ignorant of the phenomenon of "counter-gradient isotope fluxes". Perhaps Dr. Eugster could provide references for these observations (?). Otherwise, it seems that this issue is beyond the scope of the present manuscript, but could be worthy of future investigation.

The author has made no changes as a result of this comment, but remains interested in learning more about "counter-gradient isotope fluxes".

3. It would be appreciated to reword some passages where plant physiologists and plant ecologists are non-neutrally qualified as partially ignorant scientists. I must admit that I had a private discussion with Graham Farquhar at a conference in Interlaken more than 10 years ago about "counter-gradient isotope fluxes" and actually had the feeling that it is fruitful in interdisciplinary work to exchange ideas between disciplines, but should not consider ourselves superior to those who start to dig into new terrain (from their perspective) – we tend to leave a similarly bleak trace if we dare to lean outside of our own territory. I think it is the strength of interdisciplinary researchers that they take the risk to be considered a non-savant outside their area of profound expertise, and we should restrain from spreading bad marks to others from other disciplines (this relates mostly to lines 394–395, 410, 415–424).

The author is aware that some readers find his writing style to be offensive, has worked hard to try to correct this problem, and is not surprised to find that it persists. There can be little doubt regarding the benefits of exchanging ideas between disciplines, and any specific recommendations would be very welcome regarding how to reword passages so as to be more neutral.

However, having said that, the author is unwilling to refrain from criticizing theories or procedures that are incorrect. The objective here is not to consider anyone as superior, but rather to discover and defend the truth, and the author has taken great care to do this based solidly on fundamental physical laws. From the author's point of view, Jarman (1974) neglected momentum conservation, erroneously classified the description of Parkinson and Penman (1970) as "incorrect" (twice) or "substantially incorrect" (twice more), and misled an entire community of scientists along a mistaken path for several decades. The key question then is how correct this error and prevent its further propagation.

In an attempt to comply with Dr. Eugster's suggestion and avoid defaming a particular discipline, the author has adopted the following changes

- *Lines 419: delete ",as has been neglected by the discipline of plant physiology, or ecophysiology";*
- *Line 434: delete ", but broadly neglected in the field of ecophysiology"*
- *Line 447: delete "among plant physiologists"*

4. The conclusions end with a very general take-home message, but since the author puts so much emphasis in his text to educate plant ecologists, it would be beneficial to have a more specific recommendation set for what plant ecologists finally are supposed to do with this new-gained knowledge. This does not explicitly become clear and the paper would benefit by having such explicit, specific recommendations that I and other could easily pik up, understand, and implement in our own calculations.

Unfortunately, the author has not been able to develop a "quick fix": an algorithm or equation that would immediately correct WUE or c_i by accounting for non-diffusive transport. This therefore falls under the category of future research, and is open to any scientist who may have better ideas about solving these tricky issues. *The author has made no changes as a result of this comment.*

"Minor technical issues":
L. 86: add "vertical" before velocity

The author has made this change (now line 89).

L. 403: this appears to be the old notation of the previous (internal) version and should now read (w $|_0$)

The author has made this change (now line 427). The author found and corrected the same problem in the last paragraph of the discussion (now line 505).

Replies to Anonymous Referee #3

General comments: Vertical velocity has a tiny magnitude near surface and is difficult to measure because its magnitude is usually smaller than errors. However, vertical velocity plays a substantial role in mass and energy exchanges between land and atmosphere. For simplicity, they usually assume it is zero at surface. The author argues that it is non-zero by a "thought experiment". The author is a theoretical thinker. This paper shines light on this knowledge gap. I recommend it to be published with minor revision.

The author thanks the referee for this endorsement. The *author has made no changes as a result of this comment.*

Specific comments:

(1) 2.1.2 The 0th Law of Thermodynamics – I do believe that this is a case from second law of thermodynamics (Postulate of Clausius, see Thermodynamics by Enrico Fermi, 1936). I don't think that "The 0th Law of Thermodynamics" is independent from second law of thermodynamics. So I suggest using the second law of thermodynamics instead of the 0th Law so that your statements no matter heat transfer and mass diffusion are govern by the same second law of thermodynamics. Fourier's law and Fick's law are empirical relationships between fluxes and gradients. Gradients are drivers for fluxes and consequences of fluxes reduce gradients, following a single irreversible direction (entropy increasing) – equilibrium (entropy maximum) –second law of thermodynamics.

The 1936 textbook cited, although authored by a great physicist, is nonetheless out of date regarding this issue. Modern physics texts (e.g., Giancoli, 1984) recognize that the 1st and 2nd Laws, although definitively stated first, logically depend on the prior assertion of the 0th Law; this explains its odd name. Formalization of the 0th Law occurred in the mid-1930s, but was not broadly accepted until well after the publication of Fermi's textbook. The *author has made no changes as a result of this comment.*

(2) Vertical velocity at surface is always positive (upward) predicted by the equation (4). Based on your thought experiment, this looks true everywhere (leaves, ground, water surface) including large scale (e.g. synoptic scale). To my knowledge, it is sure that vertical velocity is negative in high pressure system areas and positive in low pressure system areas. Therefore, it is difficult for me to understand the positive vertical velocity predicted by your theory in high pressure system areas or divergent air-flow near surface at any scale. Please clarify the conflict in your revision.

Synoptic-scale velocities are of order 3 cm s^{-1} (e.g., Carlson and Stull, 1986, Subsidence in the nocturnal boundary layer, *J. Clim. Appl. Met.*, **25**, 1088-1099). Whether the boundary condition at the surface is the traditionally conceived $\mathbf{w}\big|_0 = 0$ cm s^{-1}, or 0.000031 cm s^{-1} as derived from eq. (4), synoptic-scale subsidence implies a convergence in the vertical winds between the surface and the height at which it occurs. It seems that there is no conflict that requires clarification. The *author has made no changes as a result of this comment.*

(3) Page 6 second paragraph, It is fine to me with "vertical advection" because it is clearly defined by vertical component It does not need to assume horizontal homogeneity.

The author agrees, and initially proposed simply to delete the last two sentences of this paragraph. However, upon careful reconsideration, the author wishes to finish this paragraph with a statement to clarify a key distinction between advection and diffusion. *Therefore, the author has deleted the two sentences at the end of the first paragraph of section 2.3 (and consequentially the Rannik et al., 2009 paper has been removed from the references section), and added a new sentence to finish this paragraph (now at lines 213-214), to say that:* "Thus advection, unlike diffusion, is not a form of transport, but rather a consequence of differential transport."

The boundary condition for the vertical velocity and its interdependence with surface gas exchange

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Abstract. The law of conservation of linear momentum is applied to surface gas exchanges, employing scale analysis to diagnose the vertical velocity (w) in the boundary layer. Net upward momentum in the surface layer is forced by evaporation (E) and defines non-zero vertical motion, with a magnitude defined by the ratio of E to the air density, as $w = \frac{E}{\rho}$. This is true even right down at the surface where the boundary condition is $w|_0 = \frac{E}{\rho|_0}$ (where $w|_0$ and $\rho|_0$ represent the vertical velocity and density of air at the surface). This Stefan flow velocity implies upward transport of a non-diffusive nature that is a general feature of the troposphere but is of particular importance at the surface, where it assists molecular diffusion with upward gas migration (of H_2O, e.g.) but opposes that of downward-diffusing species like CO_2 during daytime. The definition of flux-gradient relationships (eddy diffusivities) requires rectification to exclude non-diffusive transport, which does not depend on scalar gradients. At the microscopic scale, the role of non-diffusive transport in the process of evaporation from inside a narrow tube – with vapour transport into an overlying, horizontal air stream – was described long ago in classical mechanics, and is routinely accounted for by chemical engineers, but has been neglected by scientists studying stomatal conductance. Correctly accounting for non-diffusive transport through stomata, which can appreciably reduce net CO_2 transport and marginally boost that of water vapour, should improve characterizations of ecosystem and plant functioning.

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1 Introduction

The vertical velocity (w) is a key variable in the atmospheric sciences, whose precise diagnosis is essential for numerous applications in meteorology. Above the boundary layer, the weather is largely determined by adiabatic adjustments to vertical motion that is slight compared to horizontal winds. Closer to the surface, even a tiny w can result in relevant transport; for example, in a typical boundary layer – with representative temperature ($T = 298K$), pressure ($p = 101325$ Pa), and CO_2 mass fraction (607 mg kg^{-1}; a molar ratio of about 400 ppm) – just 61 μm s^{-1} of average vertical velocity is needed to waft a biologically significant 44 μgCO_2 m^{-2} s^{-1} (a CO_2 molar flux density of 1 μmol m^{-2} s^{-1}). Modern anemometry cannot resolve such miniscule airflow (Lee, 1998), and generally w is immensurable at many scales so that it must be derived from other variables (Holton, 1992). Such diagnostic estimation is traditional in synoptic meteorology, but has been developed less rigorously near the surface boundary.

The characterization of boundary conditions for state and flow variables, in order to enable atmospheric modelling at larger scales, is a fundamental goal of micrometeorology. Since w is an air velocity, its boundary condition $w|_0$ describes the surface-normal or vertical motion of the gas molecules found closest to the surface (at some height $z|_0$, very nearly but not exactly zero). The Navier-Stokes equations, when applied to the lower atmosphere, are particularly sensitive to the conditions specified at the boundary (Katul et al., 2004), and this lends great importance to $w|_0$ in the context of dynamic modelling. Nevertheless, until now $w|_0$ has received inadequate attention in boundary-layer meteorology.

Micrometeorologists have made presuppositions regarding $w|_0$ without formal justification and in contradiction to deductions from classical mechanics. The traditional hypothesis about near-surface winds is that they flow parallel to underlying terrain (Kaimal and Finnigan, 1994;Wilczak et al., 2001) and vanish at the surface (Arya, 1988), implying $w|_0 = 0$. This assumption underlies many derivations and abets the prevailing belief that vertical exchanges are accomplished purely by molecular diffusion within a millimeter of the surface (Foken, 2008), or purely by turbulent diffusion at heights of meters or more within the atmospheric boundary layer. However, such a premise is inconsistent with the fact of net surface gas exchange (predominantly evaporative), which implies Stefan flow with a mean velocity component normal to the surface. Net mass transfer across a surface results in a velocity component normal to the surface, and an associated non-diffusive flux in the direction of mass transfer (Kreith et al., 1999). The existence and relevance of Stefan flow – first derived and described in the 19th century – is certain. Indeed, engineers necessarily account for its role in heat and mass transfer (Abramzon and Sirignano, 1989) when precisely controlling industrial processes that include phase change, such as combustion. For these reasons, it is to be expected that a more accurate means of estimating $w|_0$ for the atmospheric boundary layer can be achieved by rigorous examination of known surface flux densities in the light of physical laws.

The remaining sections of this work aim to diagnose a defensible lower boundary condition for the vertical velocity ($w|_0$) and to interpret its significance. Section 2 presents the theory, and illustrates types of mass transport and heat exchange in fluids via an example from the liquid phase. In Section 3, an analytical framework is established and conservation of linear momentum is applied to derive $w|_0$ from published magnitudes of surface gas exchanges, demonstrating that it is directly proportional to the evaporative flux density (E), consistent with the findings of Stefan. The derived vertical velocity is seen to be relevant in defining the mechanisms of gas transport, which is not accomplished by diffusion alone – even at the surface interface. Section 4 highlights the need to rectify flux-gradient relationships by taking into account the non-diffusive component of transport; this includes boundary-layer similarity theory and physiological descriptions of stomatal conductance. Thus, the implications of these analyses are broad and interdisciplinary.

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2

76 **2 Theory**

77 The objective of this section is to establish the theoretical bases for the analyses and interpretations that
78 follow. It opens with a list of symbols (Table 1) along with the meaning and S.I. units of each variable
79 represented, and finishes with a summary of the most salient points regarding physical laws and transport
80 mechanisms to be recalled in Section 3.

81 **2.1 Relevant Scientific Laws**

82 **2.1.1 The Law of Conservation of Linear Momentum**

83 The principle of conservation of momentum is most fundamental in physics, more so than even Newton's
84 1st Law (Giancoli, 1984). It defines the momentum of a system of particles as the sum of the momenta of
85 the individual components, and establishes that this quantity is conserved in the absence of a net external
86 force. Accordingly, in atmospheric dynamics (Finnigan, 2009) a system may be defined as the N
87 component gas species comprising a particular mass of air, with a net vertical flux density of

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88 $$w\rho = \sum_{i=1}^{N} w_i \rho_i. \qquad (1)$$

89 In Eq. (1), w and ρ represent the vertical velocity and density of air, respectively, while w_i and ρ_i are the
90 properties of component i, whose species flux density is $w_i\rho_i$. For this species i, total transport $w_i\rho_i$ can

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91 be attributed to mechanisms that are diffusive (if $w_i \neq w$), non-diffusive (if $w \neq 0$), or more generally a
92 combination of these two types of transport. Dividing Eq. (1) by the net air density defines the system's
93 vertical velocity as a weighted average of its components (Kowalski, 2012), where the weighting factors

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94 are the species' densities.

95 **2.1.2 The 0th Law of Thermodynamics**

96 The 0th Law establishes the temperature as the variable whose differences determine the possibility for
97 heat exchange between thermodynamic systems. For two systems in thermal contact, if they have the
98 same temperature then they are in thermodynamic equilibrium and therefore exchange no heat. If their
99 temperatures differ, then heat will be transferred from the system with the higher temperature to that with
100 the lower temperature. Heat transfer by molecular conduction depends on gradients in the temperature; in
101 compressible fluids like air, however, turbulent diffusion can occur without thermal contact and yet bring
102 about heat transfer as determined by gradients in the potential temperature (Kowalski and Argüeso,
103 2011), accounting for any work done/received during the expansion/compression associated with vertical
104 motions.

105 **2.1.3 Fick's 1st Law of Diffusion**

106 Molecular diffusion has no effect on the net fluid momentum, but "randomly" redistributes fluid
107 components and can cause different species to migrate in different directions, according to component
108 scalar gradients. Regrettably, scientific literature contains inconsistencies regarding the scalar whose
109 gradient determines diffusion in the gas phase (Kowalski and Argüeso, 2011). The proper form of Fick's
110 1st Law for diffusion in the vertical direction is

$$F_{i,M} = -\rho K \frac{\partial f_i}{\partial z},\qquad\qquad\qquad\qquad\qquad (2)$$

where $F_{i,M}$ is the vertical flux density of species i due to molecular diffusion, which is proportional to the vertical gradient in that species' mass fraction (f_i; Bird et al. (2002)), and z is height. Also relevant are the fluid density (ρ) and molecular diffusivity (K). However, ρ must not be included in the derivative in Eq. (2), unless for the trivial case where it is constant (as in an incompressible fluid); in compressible media, gradients in gas density can arise, with no direct relevance to diffusion, due to gradients in pressure or temperature as described by the Ideal Gas Law. It is relevant to note that Adolf Fick arrived at this law, not by experimentation, but rather by analogy with Fourier's law for heat conduction (Bird et al., 2002). By the same analogy, the product of the diffusivity with the scalar gradient in Eq. (2) yields a kinematic flux, which requires multiplication by the fluid density in order to yield the flux density of interest.

Fluxes due to molecular diffusion are referenced to the motion of the fluid's centre of mass, or "mixture velocity" (Bird et al., 2002). The simplest example to describe this is that of binary diffusion where only two species compose the fluid, as in the traditional meteorological breakdown of air into components known as dry air and water vapour. In the case of "static diffusion", the fluid velocity is zero and the mass flux of one gas species (water vapour) counterbalances that of the other (dry air). When diffusion occurs in a dynamic fluid (non-zero velocity), then overall transport must be characterized as the sum of diffusive and non-diffusive components.

Turbulent diffusion is analogous to molecular diffusion in the sense that fluid components are randomly redistributed, with different species migrating as a function of gradients in their mass fractions. The primary difference is that eddies rather than molecular motions are responsible for mixing, and the eddy diffusivity (the value of K in Eq. (2), describing "K-theory" (Stull, 1988)) is a property of the flow rather than the fluid. The Reynolds number describes the relative importance of molecular and turbulent diffusion, which are otherwise indistinct with respect to the analyses that follow, and will simply be grouped and referred to as "diffusive transport".

2.2 Transport processes

In this section, two case studies from the liquid phase will help identify and define non-diffusive and diffusive types of transport, as well as their scalar source/sink determinants. Let us consider the case of freshwater ($35 \cdot 10^{-5}$ mass fraction of salt) with constant temperature and composition flowing through a tube into the bottom of a pool (Figure 1) of salinity specified according to the two case scenarios defined below. Considering only flow within the tube (at point 1), whether laminar or turbulent, it clearly realizes non-diffusive transport of salt, since the salt has no particular behaviour with respect to the fluid, but simply goes with the flow. There are no scalar gradients within the tube, and so there is neither diffusion nor advection. Let us now describe diffusive transport processes within the pool (at point 2), and the nature (whether absolute or relative) of the relevant fluid properties whose gradients determine them by defining sources/sinks, using two illustrative case scenarios.

156   The temperature is constant in time and space, but other *characteristics of the two case scenarios* are

157   chosen to elucidate the relationship between diffusive transport processes and scalar gradients:

158   1) *Due to surface evaporation that balances the mass input from the tube, the pool mass is constant; the*

159   *water is maintained isothermal by surface heating that supplies the (latent) energy for evaporation.*

160   *Initially (t_0) the pool has zero salt mass, but salinity increases constantly, equalling that of the tube*

161   *water at some moment (t_{eq}) and rising by another two orders of magnitude to reach that of sea water*

162   *($35 \cdot 10^{-3}$) by the end of the scenario (t_f).* This case is of interest from both <u>(a)</u> salt/solute and <u>(b)</u>

163   thermodynamic points of view:

164   a)   In solute terms, the tube represents a source of (absolute) salt to the pool, but not always of

165   (relative) salinity. Initially (t_0), the water from the tube is more saline than that in the pool, such

166   that non-diffusive and diffusive transport processes operate in tandem to transport salt from the

167   tube upward into the pool; at this moment, the tube is a source of salinity. Salinity advection,

168   defined as the negative of the inner product of two vectors (the velocity with the salinity

169   gradient, with opposite signs), is then positive. Ultimately however (at $t > t_{eq}$), the water in the

170   pool is more saline than that entering from the tube, such that non-diffusive and diffusive salt

171   transport are in opposite directions; then the tube dilutes the pool and is a salinity sink, but still a

172   salt source. Salinity advection at t_f is negative. The pool continues to gain salinity after t_{eq},

173   despite the diluting effects of the tube, due to the concentrating effects of evaporation, which is

174   the ultimate source of salinity. This distinction matters because the gradients that drive advection

175   and diffusion are those in salinity, a relative (not absolute) salt measure. At t_f, the diffusive

176   salinity fluxes are oriented against the flow within the pool (downward, and radially inward

177   towards the diluting tube, despite its being a net salt source). By contrast, non-diffusive transport

178   always goes with the flow, and accounts for continued upward and outward salt transport,

179   increasing the salt content at the surface.

180   b)   Although thermodynamically trivial – with no heat exchanges whatsoever within the water as

181   determined by the 0th Law – this case nonetheless illustrates the nature of the scalars that

182   determine heat transfer by advection and diffusion (conduction). The "heat content" of the pool

183   decreases as it becomes more and more saline, due to the inferior heat capacity of saltwater

184   versus freshwater. Similarly, salt diffusion/advection is initially upward/positive but ultimately

185   downward/negative, yet the corresponding implications regarding heat content fluxes say

186   nothing about the transfer of heat. The point here is that the dynamics of the heat content must

187   not be interpreted in terms of heat fluxes, which was done by Finnigan et al. (2003). For this

188   reason, meteorologists correctly define "temperature advection" (Holton, 1992) based the

189   thermodynamic relevance of gradients in the variable singled out by the 0th Law.

190

191   2) *Let us now specify that the water in the pool has the same (freshwater) salinity as that coming from*

192   *the tube ($35 \cdot 10^{-5}$). If we furthermore remove both surface evaporation and heating from scenario (1),*

193   *then the temperature remains constant and the salinity corresponds uniformly to that of freshwater,*

194   *but the pool accumulates mass.* In this case, there are convergences in the non-diffusive transports of

195   water, salt, and heat content: fluxes into the pool are positive, while fluxes out are null. However,

196	there are no gradients in temperature or salinity, and so there is neither diffusion nor advection in this
197	scenario. The pool does gain volume (depth) but this is only because the fluid under consideration is
198	incompressible. By contrast, for the gas phase, accumulation of absolute quantities – such as air and
199	trace constituent mass, and heat content – can occur in a constant volume context (e.g., "at a point",
200	in an Eulerian fluid specification) due to convergent, non-diffusive transport that defines
201	compression. In the pool, diffusion and advection are clearly null because they are determined by
202	gradients in the relative trace gas amount – the mass fraction –, a variable of essential utility for the
203	gas phase because it is immune to the effects of compression.

204

2.3 An advection-diffusion synopsis

The analyses that follow rely on the succeeding key points drawn from sections 2.1 and 2.2. Advection and diffusion depend on gradients in scalars whose nature is relative rather than absolute. In incompressible thermodynamics, the relevant gradients are those in the temperature, and not the heat content. For trace constituents, the relevant scalar is the mass fraction (e.g., salinity) and not the species density. Advection and diffusion are otherwise physically very distinct. Like non-diffusive transport, diffusion is a vector whose vertical component is of particular interest in the context of surface-atmosphere exchange. By contrast, advection is a scalar; for some arbitrary quantity ξ, it is defined as the negative of the inner product $\mathbf{v} \cdot \nabla \xi$, where $\mathbf{v}$ is the fluid velocity and ∇ is the gradient operator. Thus advection, unlike diffusion, is not a form of transport, but rather a consequence of differential transport.

The scenarios depicted above correspond to the incompressible case (liquid). When the effects of compressibility are irrelevant, it can be convenient to add the incompressible form of the continuity equation ($\nabla \cdot \mathbf{v} = 0$) to advection yielding $-\nabla \cdot \xi \mathbf{v}$, the convergence of a kinematic flux. This is called the "flux form" of advection. For a compressible medium such as the atmosphere, however, if ξ is taken to represent some "absolute fluid property such as the (gas) density" (Finnigan et al., 2003), then the transformation of advection into flux form cannot be justified (Kowalski and Argüeso, 2011), since using the incompressible form of the continuity equation leads to unacceptable errors in conservation equations for boundary-layer control volumes (Kowalski and Serrano-Ortiz, 2007). By contrast, the expression of advection in flux form can be valid if the scalar ξ is carefully chosen for its immunity to the effects of compression, as is the case for the mass fraction. These generalizations regarding the nature of transport by non-diffusive and diffusive mechanisms, and also the nature of advection, will now be applied to the case of vertical transport very near the surface and the mechanisms that participate in surface exchange, after first deriving the boundary condition $w|_0$.

3 Analysis

3.1 Framework

The analysis will focus on a system defined as a mixture of gas molecules of different species, whose momentum will be examined. The system's mass is defined (Table 2) by gas components in a ratio that

6

Eliminado: To be clear, it can make sense to speak of "upward diffusion", but certainly not "upward advection". The tendency, in the science of surface-atmosphere exchange, to speak of "vertical advection" (e.g., Rannik et al., 2009) is intimately related to an assumption of horizontal homogeneity, precluding horizontal scalar gradients particularly in the direction of the mean wind.

243 corresponds quite closely to that of the atmosphere (Wallace and Hobbs, 2006) but updated to more
244 closely reflect actual atmospheric composition. At a representative ambient temperature (T = 298 K) and
245 pressure (p = 101325 Pa), the many millions of molecules forming this system occupy a volume of 10^{-15}
246 m^3 with 70% relative humidity. The system geometry will be specified in four different ways, according
247 to the different spatial scales for which $w|_0$ is to be described:

    A. At the synoptic scale, the volume occupied by the system is a lamina of depth $\delta z \sim 10^{-27}$ m,
        bounded above and below by constant geopotential surfaces, with horizontal dimensions (Δx and
        Δy) on the order of 10^6 m. The fact that δz is thinner than the dimension of a molecule matters
        not at all when classifying any and all molecules whose centres of mass (points, with neither size
        nor dimension) occupy the lamina as belonging to the volume;

    B. At the micrometeorological scale, the volume overlies a flat surface and is shaped as a
        rectangular lamina of depth $\delta z \sim 10^{-21}$ m, with horizontal dimensions (Δx and Δy) of 10^3 m;

    C. At the leaf scale, the volume is a rectangular lamina of depth $\delta z \sim 10^{-11}$ m, with horizontal
        dimensions (Δx and Δy) of 10^{-2} m; and

    D. At the microscopic scale of plant stomata, the volume is a cube with $\Delta x = \Delta y = \delta z = 10^{-5}$ m. For
        the purpose of transitioning between the leaf and microscopic scales, plant pores are assumed to
        occupy a stomatal fraction σ of the leaf surface and yet accomplish all gas exchange, with the
        remaining fraction (1-σ) occupied by a cuticular surface whose gas exchange is assumed to be
        null (Jones, 1983).

262 Independent of scale, the base height $z|_0$ of the volume is the lowest for which only air – and neither
263 ocean wave nor land surface element – occupies the volume. The land/ocean/leaf surface will be assumed
264 to be static (i.e., its vertical velocity is zero), impenetrable to the wind (explicitly neglecting ventilation of
265 air-filled pore space), smooth, level and uniform, all for the sake of simplicity. The temporal framework
266 for the analysis is instantaneous, with no need to choose between Eulerian and Lagrangian fluid
267 specifications.

269 The direction of momentum transport to be examined is vertical, meaning perpendicular to constant
270 geopotential surfaces and therefore to the underlying surface. At the stomatal scale, the stoma to be
271 examined is situated on the upper side of a flat, horizontal leaf; water vapour exiting the stomatal aperture
272 during transpiration therefore has a positive vertical velocity. These analyses can be generalized to
273 sloping surfaces and/or stomata on the underside of leaves, simply by referring to the "surface-normal"
274 rather than "vertical" velocity. Hereinafter, however, the term "vertical" will be employed for
275 conciseness.

3.2 The vertical velocity at the surface boundary

278 Knowledge regarding surface exchange (gas flux densities) has advanced to the point where the boundary
279 condition for the vertical velocity ($w|_0$) can be estimated from conservation of linear momentum –
280 applying Eq. (1) to the system defined in Table 2 –, and vastly simplified to a simple function of the
281 evaporation rate (E). The species flux densities ($w_i \rho_i$) within the system represent the surface exchanges

of the corresponding gas species (i). Scale analysis of surface gas exchange magnitudes, published from investigations at a particularly well-equipped forest site in Finland (Table 3), reveals that for the water vapour species (i=4), the flux density ($E = w_4\rho_4$) is orders of magnitude larger than both the flux density of any dry air component species and even the net flux density of dry air. Such dominance by water vapour exchanges is representative of most surfaces worldwide. This is especially so because the two largest dry air component fluxes are opposed, with photosynthetic/respiratory CO_2 uptake/emission largely offset by O_2 emission/uptake (Gu, 2013). Hence, following tradition in micrometeorology (Webb et al., 1980), dry air exchange can be neglected, allowing the elimination from Eq. (1), when applied at the surface, of all species flux densities except for that of water vapour (H_2O; i=4). Therefore, net air transfer across the surface can be approximated very accurately as

$$w|_0\, \rho|_0 = w_4|_0\, \rho_4|_0 = E \,, \tag{3}$$

where $w_4|_0$ and $\rho_4|_0$ are the H_2O species velocity and density at the surface. Equation (3) states that, at the surface, the net vertical flux density of air is equal to the net vertical flux density of water vapour, which is the evaporation rate. Solving this for $w|_0$ allows estimation of the lower boundary condition for the vertical velocity as

$$w|_0 = \frac{E}{\rho_4|_0} \,. \tag{4}$$

The representative evaporation rate prescribed in Table 3 and vertical velocity resulting from eq. (4) are valid for most of the scales defined above. Thus, the boundary condition $w|_0$ is valid for the synoptic scale (notwithstanding vertical motion aloft, such as subsidence), for the micrometeorological scale, and even for the leaf scale. In the context of scale analysis, leaves may be approximated as having equal area as the underlying surface (i.e., a unit leaf area index, or LAI=1), and equal evaporation rates as the surface in general. This latter assumption does not neglect soil evaporation, but only excludes the possibility that it dominate leaf evaporation by an order of magnitude. Thus, it will be assumed here that the assumed evaporation rate and derived vertical velocities are equally valid at synoptic (A), micrometeorological (B), and leaf (C) scales. The order of magnitude is different, however, at the microscopic (D) scale. To show this, it will be assumed here that all leaf evaporation (or transpiration) occurs through the small fraction of the leaf that is stomatal (σ), such that both the stomatal evaporative flux density and the lower boundary condition for the vertical velocity ($w|_0$) are a factor $1/\sigma$ greater than that at larger scales. Independent of scale, Eq. (4) states that, for a positive evaporation rate, the boundary condition for the vertical velocity is non-zero and upward.

Given that the surface boundary is static, it may well be asked why there is a non-zero boundary condition for the vertical velocity of air. The answer is that evaporation induces a pressure gradient force that pushes air away from the surface. Evaporation into air increments the water vapour pressure and thereby the total pressure, according to Dalton's law. If evaporation were to proceed until achieving equilibrium, the pressure added by evaporation would correspond to the saturation vapour pressure (e_s; Figure 2), whose temperature dependency has been quantified empirically and is described by the Clausius-

325 Clapeyron relation. It is this evaporation-induced pressure gradient force that pushes the manometer in
326 Fig. 2 to its new position, and similarly that drives winds away from the surface.

327

328 Although this upward air propulsion occurs at the surface, air velocities are generally upward throughout
329 the boundary layer in a climatological context. Indeed, the dominant role of water vapour in determining
330 the net vertical momentum of air is a general feature of the troposphere. In the context of the
331 hydrological cycle, water vapour is transported from the surface where it has an evaporative source, to
332 further aloft where clouds develop via processes that act as water vapour sinks: condensation and vapour
333 deposition onto ice crystals (or ice nuclei). In terms of total water, upward transport in the gas phase is
334 offset, over the long term, by downward transport in liquid and solid phases (e.g., rain and snow); unlike
335 the water vapour flux, however, precipitation does not directly define air motion. It is true that downward
336 water vapour transport occurs during dewfall – with surface condensation, as described by Eq. (4) with a
337 negative evaporation rate ($E < 0$) –, but this plays a minor role in the global water balance. Generally, the
338 relative magnitudes of gas exchanges used for the scale analysis in Table 3 are representative throughout
339 most of the troposphere, with upward water vapour flux densities dominating those of other gases in the
340 vertical direction. In the surface layer, sometimes termed the "constant flux layer" (Dyer and Hicks,
341 1970), Eq. (4) can be extrapolated away from the surface under steady-state conditions to yield

342 $$w = \frac{E}{\rho}.$$ (5)

343

3.3 Mechanisms of gas transport at the surface

345 Non-zero vertical momentum in the lower atmosphere and right at the surface boundary – dominated by
346 the flux density of water vapour and generally upward due to evaporation – means that diffusion is not the
347 lone relevant transport mechanism that participates in surface exchange, as has been generally supposed.
348 This is true for all atmospheric constituents, and not only for water vapour; over an evaporating surface,
349 any molecule undergoing collisions with its neighbours does not experience a random walk (a
350 characteristic of static diffusion), but rather tends to be swept upward with the flow. The upward air
351 current similarly wafts aerosol particles, although these may move downwards if their fall velocities
352 exceed the upward air motion. The upward flow velocity is rather small – just 31 μm s^{-1} for the conditions
353 specified above and the evaporation rate of Table 3, according to Eqs. (4) and (5). It does not exclude the
354 possibility of diffusive transport in any direction, but does imply a relevant, non-diffusive component of
355 transport for any gas, whose magnitude is not related to that gas's scalar gradient.

356

357 The non-diffusive flux density of species i can be expressed as

358 $$F_{i,non} = w\rho_i ,$$ (6)

359 and when substituting for w from Eq. (5) this becomes

360 $$F_{i,non} = E f_i ,$$ (7)

9

361 i.e., the product of the evaporation rate and the species mass fraction. Examination of its magnitude near

362 the surface for different gases will now show that, while this is often small in comparison with the

363 diffusive component, it is not negligible in every case, depending on the magnitudes of the mass fraction

364 and surface exchange for the gas considered.

365

366 Interpreting decomposed transport is simplest when examining a gas whose surface exchange is very well

367 known, such as the null value for inert Argon (Ar) that constitutes ca. 1.3% of dry atmospheric mass

368 (Wallace and Hobbs, 2006). Considering the state variables defined by Table 2 and the evaporation rate

369 of Table 3, Eq. (7) indicates 458 μg m^{-2} s^{-1} (a molar flux density of 11.6 μmol m^{-2} s^{-1}) of upward, non-

370 diffusive Ar transport ($F_{3,non}$). To comprehend this, it helps to recall that the constant addition of H_2O

371 dilutes dry air at the surface and promotes its downward diffusion. For a null net flux of inert Ar to exist,

372 downward diffusion of this dry air component must exactly cancel the upward non-diffusive transport,

373 and therefore is 458 μg m^{-2} s^{-1} for the state and evaporative conditions specified above. These opposing

374 non-diffusive and diffusive Ar transport processes are quite analogous to case scenario 1 of Section 2.2, at

375 the instant t_f when the fluid emitted into the pool has a diluting effect. Such dual transport mechanisms

376 are also relevant for vital gases, with different transport directions and degrees of relevance, depending on

377 the density and flux density of the gas in question.

378

379 For H_2O, the two types of gas transport mechanisms operate in tandem, with the non-diffusive component

380 contributing a fraction of upward H_2O transport that, according to Eq. (7), is exactly the water vapour

381 mass fraction or specific humidity (Wallace and Hobbs, 2006)

382 $$q \equiv f_4 \equiv \frac{\rho_4}{\rho}. \tag{8}$$

383 This is just 2% for the state conditions previously specified, but can approach 5% for very warm

384 evaporating surfaces and/or high-altitude environments. The breakdown of H_2O transport into diffusive

385 and non-diffusive components is analogous to case scenario 1 of Section 2.2 at an instant prior to t_{eq} when

386 the fluid introduced to the pool is highly concentrated, in comparison with the fluid already in the pool. In

387 any case, non-diffusive H_2O transport is generally secondary to diffusive transport, but its neglect in an

388 ecophysiological context can lead to larger relative errors, as will be shown in Section 4.

389

390 For CO_2, which usually migrates downward during evaporative conditions because of photosynthetic

391 uptake, upward transport of a non-diffusive nature is even more relevant, opposing the downward flux

392 due to diffusion. To see this, let us examine the typical gas transport magnitudes of Table 2 and the

393 atmospheric state conditions specified above. According to Eq. (7), non-diffusive CO_2 transport ($F_{5,non}$) is

394 then 21.5 μg m^{-2} s^{-1} (a molar flux density of 0.49 μmol m^{-2} s^{-1}) in the upward direction, requiring that

395 downward CO_2 diffusion be 109.5 μg m^{-2} s^{-1} in order to yield 88 μg m^{-2} s^{-1} of net surface uptake; if not

396 accounting for the non-diffusive resistance to net transport, the CO_2 diffusivity would be underestimated

397 by ca. 20%. The case of CO_2 uptake is not analogous to any pool/tube scenario in Figure 1. However,

398 different conditions with equal evaporation ($E = 36$ mg m^{-2} s^{-1}) and CO_2 *emission* in the amount of 21.5

399 μg m^{-2} s^{-1} (by respiration, for example) would correspond to the case of zero CO_2 diffusion (as at the

400 instant t_{eq}), since the CO_2 mass fractions of both the atmosphere and the gas mixture emitted by the

401 surface are identical. Viewed in the traditional diffusion-only paradigm, such a situation involving a net

402 flux but no gradient ($F_3 = F_{3,non}$) would require a physically absurd infinite diffusivity. At this same

403 evaporation rate, but with lower CO_2 emission, diffusion of CO_2 would be downward, towards the surface

404 which is a source of CO_2 but a sink of the CO_2 mass fraction (analogous to salinity in case scenario 1 of

405 Section 2.2 at some instant between t_{eq} and t_f when the fluid emitted to the pool has a diluting effect).

406 Whatever the direction of net CO_2 transport, these case examples demonstrate the need for sometimes

407 substantial rectifications to flux-gradient relationships – whether expressed as a conductance, resistance,

408 deposition velocity, or eddy diffusivity (K-theory), – when correctly accounting for non-diffusive

409 transport.

410 **4 Discussion**

411 Relevant transport of a non-diffusive nature implies the need to revise the basis of flux-gradient theory,

412 both in the boundary layer and also at smaller scales regarding gas transfer through plant pores. One of

413 the key goals of micrometeorology has been the derivation of the vertical transports of mass, heat, and

414 momentum from profiles of wind speeds and scalar variables in the boundary layer (Businger et al.,

415 1971). The analyses above elucidate how gradients relate to only the diffusive components of such

416 exchanges. Therefore, non-diffusive flux components must be subtracted out in order to characterize

417 turbulent transport in terms of eddy diffusivities, a key goal of Monin-Obukhov Similarity Theory

418 (Obukhov, 1971). Perhaps more important is the need to distinguish between non-diffusive and diffusive

419 transport mechanisms prior to assessing molecular diffusivities (conductances).

420

421 When Eq. (3) is applied at the stomatal apertures where virtually all plant gas exchanges occur, it is

422 revealed that jets of air escape from these pores during transpiration. In the context of the scale analysis

423 begun in Section 3.2, it is appropriate to note that even fully open stomata occupy just 1% of leaf area

424 (Jones, 1983), leaving 99% cuticular and inert with regard to vital gas exchanges ($\sigma = 0.01$). As noted in

425 Section 3.2, this means that for the microscopic scale (D; Section 3.1) of the stomatal aperture, both the

426 local evaporative flux density (E) and therefore the lower boundary condition for the vertical velocity

427 ($w|_0$) predicted by Eq. (4) are two orders of magnitude greater than the 31 μm s^{-1} estimated above. In

428 other words, a typical average airspeed exiting a stomatal aperture is 3.1 mm s^{-1}. For non-turbulent flow

429 through a cylindrical tube/aperture (i.e., Poiseuille flow), the velocity at the core of such an air current is

430 twice as large. If a characteristic time scale is defined for air blowing through stomata as the ratio of a

431 typical stomatal aperture diameter (ca. 6 μm) to this core velocity, it is found to be of order 10 ms,

432 illustrating that air is expelled from plants in the form of "stomatal jets". Non-diffusive gas transport by

433 such airflow exiting stomata – assisting with water vapour egress but inhibiting CO_2 ingress – has been

434 previously conceived.

435

436 The concept of net motion and consequent non-diffusive transport out of stomata is not new, but has been

437 disregarded by plant ecologists. Parkinson and Penman (1970) put forth that the massive water vapour

11

444 flux from transpiration implies an outbound air current as a background against which diffusion operates.
445 Regrettably, however, their interpretation has largely been forgotten, having been refuted in an analysis
446 (Jarman, 1974) that incorrectly assumed "no net flow of air" – disregarding conservation of momentum –
447 and yet seems to have gained acceptance (von Caemmerer and Farquhar, 1981). Similarly, Leuning
448 (1983) recognized the relevance of non-diffusive transport and furthermore identified excess pressure
449 inside the stomatal cavity as the impetus for the outward airstream (which he termed "viscous flow"), but
450 had little impact on the mainstream characterization of stomatal conductance. Rather, important aspects
451 of ecophysiology continue to hinge upon the assumption that diffusion alone transports vital gases
452 through plant pores, disregarding both the above-mentioned studies and more importantly the fact that gas
453 transport mechanisms through such apertures were accurately described by one of the great physicists of
454 the 19th century.

455

456 Because Josef Stefan helped substantially to establish the fundaments of classical physics, his name is
457 often mentioned in the same breath as those of Boltzmann (regarding blackbody radiation) and Maxwell
458 (for diffusion). However, his work in the latter regard has been broadly ignored by scientists studying gas
459 exchanges through plant stomata. Stefan's study of evaporation from the interior of a narrow, vertical
460 cylinder with vapour transport into an overlying, horizontal stream of air is of particular relevance to the
461 discipline of ecophysiology. He determined that this is not a problem of "static diffusion", but rather
462 includes an element of non-diffusive transport due to a mean velocity in the direction of the vapour flux,
463 induced by evaporation and now commonly known as Stefan flow. Engineers know this history, refer to
464 such a scenario as a Stefan tube (Lienhard and Lienhard, 2000), and routinely reckon transport by Stefan
465 flow in addition to that caused by diffusion. Such accounting is necessary for precise control in industrial
466 applications such as combustion, and is described in many chemical engineering texts (Kreith et al., 1999;
467 Lienhard and Lienhard, 2000; Bird et al., 2002). The phenomenon of transpiration through a stoma is a
468 reasonable proxy for a Stefan tube, the main difference being that evaporation in the Stefan tube depletes
469 the pool of evaporating liquid, whose surface therefore recedes downward. By contrast, the evaporating
470 water in the stomatal cavity is continually replenished by vascular flow from within the plant; if anything,
471 this reinforces the magnitude of the upward vertical air velocity, in comparison with the Stefan tube,
472 consistent with that derived from momentum conservation as in Eqs. (4) and (5).

473

474 Non-diffusive transport by Stefan flow has implications for defining key physiological parameters,
475 greater than the percentages of CO_2 and water vapour transport calculated above. Plant physiologists have
476 postulated that stomata act to maximise the ratio of carbon gain to water loss (Cowan and Farquahar,
477 1977) or water use efficiency (WUE), an ecosystem trait that constrains global biogeochemical cycles
478 (Keenan et al., 2013). In formulating this parameter, presuming molecular diffusion to be the lone
479 transport mechanism, the water vapour conductance is usually taken as 1.6 times that of CO_2 (Beer et al.,
480 2009), based on the ratio of their diffusivities – the inverse of the square root of the ratio of their
481 molecular masses, according to Graham's law. Such an assumption underlies the very concept of stomatal
482 control (Jones, 1983), but neglects the role of non-diffusive transport for both gases. Net momentum
483 exiting stomata both expedites water vapour egress and retards CO_2 ingress, versus the case of static

485 diffusion, in each case acting to reduce the WUE. Importantly, water vapour transport by stomatal jets

486 depends not only on physiology but also physically on the state variable q, according to Eq. (8).

487 Consistent with the determinants of q, as the temperature of a (saturated) stomatal environment increases,

488 even for a constant stomatal aperture, the WUE is reduced, wresting some control over gas exchange rates

489 from the plant. Perhaps equally importantly, opposition to CO_2 uptake by stomatal jets also should be

490 considered when modelling the most fundamental of biological processes, namely photosynthesis.

491

492 Accurate modelling of primary production in plants may require a fuller description of stomatal transport

493 mechanisms, including non-diffusive expulsion by jets. The partial pressure of CO_2 inside the stomata is a

494 key input parameter for the classic photosynthesis model (Farquhar et al., 1980), but is never directly

495 measured. Rather, it must be inferred from gas exchange measurements and assumptions about the

496 relative conductance of water vapour and CO_2, as described above. The amendment of such calculations

497 to account for non-diffusive transport of both CO_2 and H_2O should help to improve the accuracy of

498 physiological models.

499

500 As a final note regarding ecophysiology, studies of plant functioning conducted using alternative gas

501 environments should be interpreted with care. Stomatal responses to humidity variations have been

502 studied in several plant species using the $He:O_2$ gas mixture termed "helox" (Mott and Parkhurst, 1991).

503 In the context of conservation of linear momentum, it is relevant that the effective molecular weight of

504 helox is just 29% that of dry air. Under equal conditions of temperature and pressure, helox has far less

505 density, and so during transpiration both $w|_0$ from Eq. (4) and the non-diffusive component of stomatal

506 transport from Eq. (7) are 3.5 times greater than in air. The validity of helox for characterizing natural

507 plant functioning is thus dubious due to its low inertia versus that of air.

Conclusions

509 Evaporation (E) is the dominant surface gas exchange, and forces net upward momentum in the surface

510 layer such that the lower boundary condition for the vertical velocity is $w|_0 = \frac{E}{\rho|_0}$, where $\rho|_0$ is the air

511 density at the surface. This non-zero vertical velocity describes Stefan flow and implies gas exchange of a

512 non-diffusive nature, which must be extracted from the net transport of any gas prior to relating that gas's

513 resultant diffusive transport component to scalar gradients, as in Monin-Obukhov Similarity Theory. Such

514 correction of flux-gradient theory is of particular import for descriptions of gas exchange through plant

515 stomata, which should be amended to account for non-diffusive transport by "stomatal jets" that help

516 expel water vapour but hinder the ingress of CO_2

Eliminado: $[w]_0$

Eliminado: is

526

527

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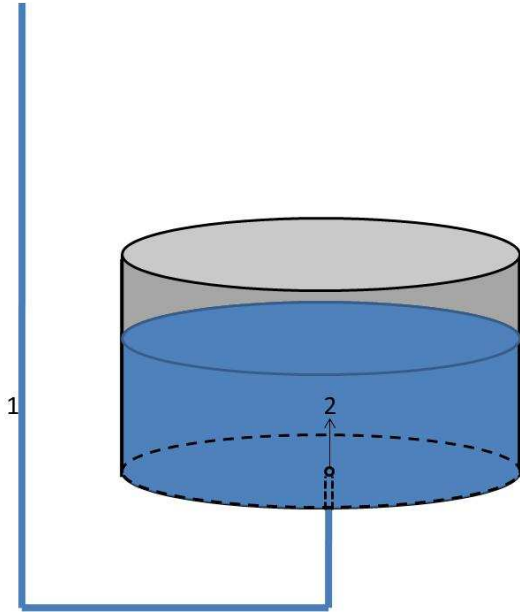
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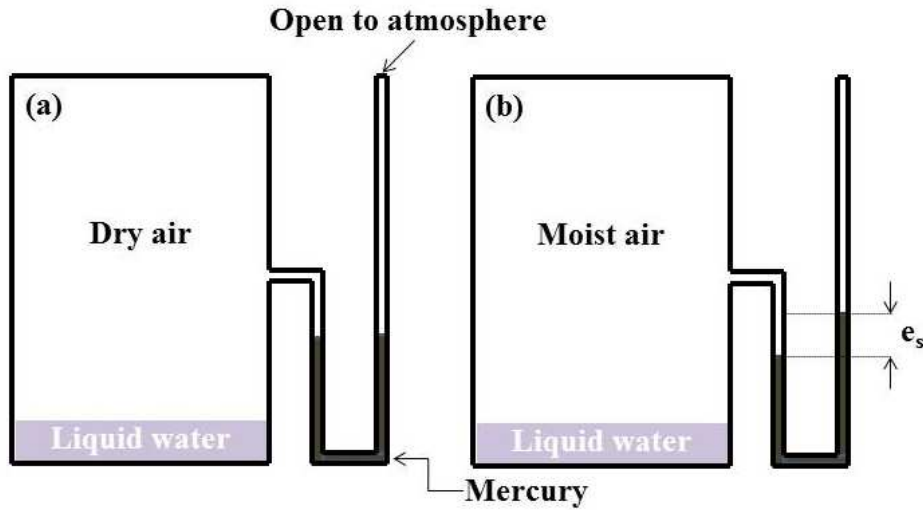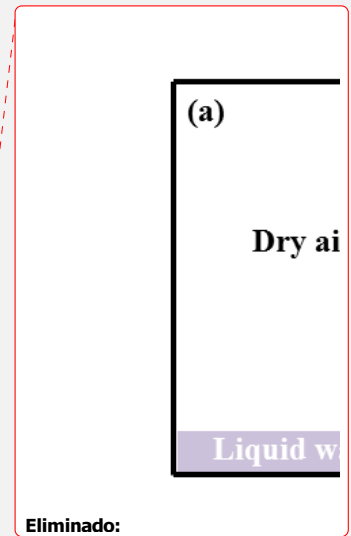609 Meteorology, 99, 127 - 150, 2001.

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616 **Figure 1: A pool of water being fed from below by a tube. The points indicate water (1) in the tube, and (2) in**
617 **the pool. The arrow indicates the direction of flow.**

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621 **Figure 2: Illustration of evaporation incrementing air pressure. Chamber air evolves from (a) dry air initially**
622 **at atmospheric pressure; to (b) moist air at a pressure that has risen by the partial pressure of water vapour,**
623 **ultimately at equilibrium (saturation vapour pressure, e_s). The force generated by evaporation propels the**
624 **mercury in the manometer from its initial position.**

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628   **Table 1: List of symbols, with their meanings and units.**

Symbol	Variable represented	S.I. Units	Tensor Order
**General Variable Representations**		
ξ	An arbitrary magnitude (can represent any scalar variable)	Depends on ξ	0 (scalar)
ξ_i	The magnitude of arbitrary variable ξ for gas species i	Depends on ξ	0 (scalar)
$\overline{\nabla\xi}$	The spatial gradient in arbitrary variable ξ	Depends on ξ	1 (vector)
$[\xi]_0$	The lower boundary condition for arbitrary variable ξ	Depends on ξ	0 (scalar)
**Specific Variable Representations**		
Δx, Δy	Horizontal dimensions of an analytical volume	m	0 (scalars)
δz	Vertical dimension (thickness) of an analytical volume	m	0 (scalar)
E	Evaporative flux density across a horizontal surface	kg m^{-2} s^{-1}	0 (component)
e_s	Saturation vapour pressure	Pa	0 (scalar)
f	Mass fraction	Non-Dimensional	0 (scalar)
F_i	Vertical flux density of gas species i	kg m^{-2} s^{-1}	0 (component)
$F_{i,non}$	Non-diffusive component of F_i	kg m^{-2} s^{-1}	0 (component)
i	Index for counting gas species (as in Table 2)	-	0 (scalar)
K	Molecular diffusivity	m^2 s^{-1}	0 (scalar)
LAI	Leaf area index	Non-Dimensional	0 (scalar)
p	Pressure	Pa	0 (scalar)
q	Specific humidity	Non-Dimensional	0 (scalar)
ρ	Air density	kg m^{-3}	0 (scalar)
σ	Stomatal fraction of leaf area	Non-Dimensional	0 (scalar)
T	Air temperature	K	0 (scalar)
t	Time	s	0 (scalar)
t_0	Initial instant of a case scenario	s	0 (scalar)
t_{eq}	Equilibrium instant of a case scenario	s	0 (scalar)
t_f	Final instant of a case scenario	s	0 (scalar)
$\boldsymbol{v}$	Air velocity	m s^{-1}	1 (vector)
w	Vertical component of $\boldsymbol{v}$	m s^{-1}	0 (component)
WUE	Water use efficiency	Non-dimensional	0 (scalar)
z	Height above the surface	m	0 (component)

Eliminado: k

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632 **Table 2: Gas components comprising the system to be examined, and their masses.**

i	Gas	Mass (kg)
1	Nitrogen (N_2)	$9.14 \cdot 10^{-16}$
2	Oxygen (O_2)	$2.80 \cdot 10^{-16}$
3	Argon (N_2)	$1.56 \cdot 10^{-17}$
4	Water vapour (H_2O)	$1.61 \cdot 10^{-17}$
5	Carbon dioxide (CO_2)	$7.36 \cdot 10^{-19}$
6	Methane (CH_4)	$1.14 \cdot 10^{-21}$
7	Nitrous oxide (N_2O)	$5.70 \cdot 10^{-22}$
8	Ozone (O_3)	$4.01 \cdot 10^{-23}$

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635 **Table 3: The first six air components by their surface exchange scale magnitude, and the net exchange of air as**
636 **the sum of these flux densities. Representative surface exchanges are taken from the Finnish boreal forest site**
637 **(Suni et al., 2003; Aaltonen et al., 2011). The O_2 exchange rate assumes 1:1 stoichiometry with CO_2.**

gas	Typical mass flux, F_i (mg m^{-2} s^{-1})	Corresponding molar flux (mmol m^{-2} s^{-1})	Source	i
H_2O	36	2	(Suni et al., 2003)	4
CO_2	-0.088	-0.002	(Suni et al., 2003)	5
O_2	0.064	0.002	(Gu, 2013)	2
CH_4	-0.000032	-0.000002	(Aaltonen et al., 2011)	6
O_3	-0.0000096	-0.0000002	(Suni et al., 2003)	8
N_2O	0.00000088	0.00000002	(Aaltonen et al., 2011)	7
Air	35.98	-	This study	-

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