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Correspondence and Communications

Absorbable sutures for skin closure after carpal tunnel decompression: A Cochrane review summary

Dear Sir,

Carpal tunnel decompression (CTD) is the most common elective hand operation, with approximately 73,000 procedures performed annually in England.[1] After CTD, skin closure may be achieved with absorbable or non-absorbable sutures. Our Cochrane review[2] collates the evidence comparing absorbable versus non-absorbable sutures for skin closure after CTD and we have summarised our findings below.

We included five randomised trials (255 participants) from Europe. All studies were at high risk of methodological bias and the certainty of the conclusions (GRADE) from the evidence was very low. However, following open CTD, there was no difference in pain scores between absorbable and non-absorbable sutures at 10 days (standardised mean difference 0.03 [95% CI −0.43 to 0.48]; $I^2 = 43$; Fig. 1) or 6 weeks (standardised mean difference 0.06 [95% CI −0.72 to 0.84]; $I^2 = 84\%$; Fig. 2). Ten days after endoscopic CTD, pain may be slightly less with absorbable sutures (SMD −0.81 [95% CI −1.36 to −0.25]). There was no difference in the risk of wound inflammation between suture types, regardless of whether the surgery was performed open (relative risk 2.28 [95% CI 0.24 to 21.91]; $I^2 = 90\%$) or endoscopically (relative risk 0.93 [95% CI 0.06 to 14.09]). There was no difference in hand function or scar satisfaction between suture types. No adverse events (e.g. infection or bleeding) were reported.

The NHS reference cost of a nurse appointment (to remove sutures) is £68-120[3] and the risk of complications after CTD is very low[4]. Therefore, we suggest that if surgeons use absorbable sutures to close the skin after CTD and arrange no face-to-face follow-up, then the NHS could save over £5million annually. Our Cochrane review recommends further non-inferiority randomised trials which might benefit patients and the health service alike.

Competing interests

None declared

Compliance with ethical standards

Funding

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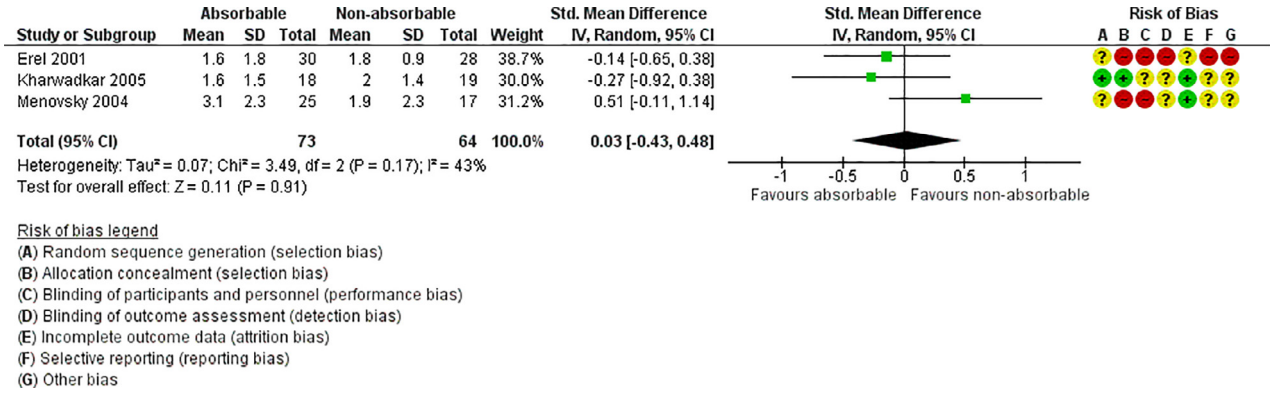

Figure 1 A forest plot of patient-reported pain 10 days following open CTD, showing no evidence of a difference between absorbable and non-absorbable sutures.

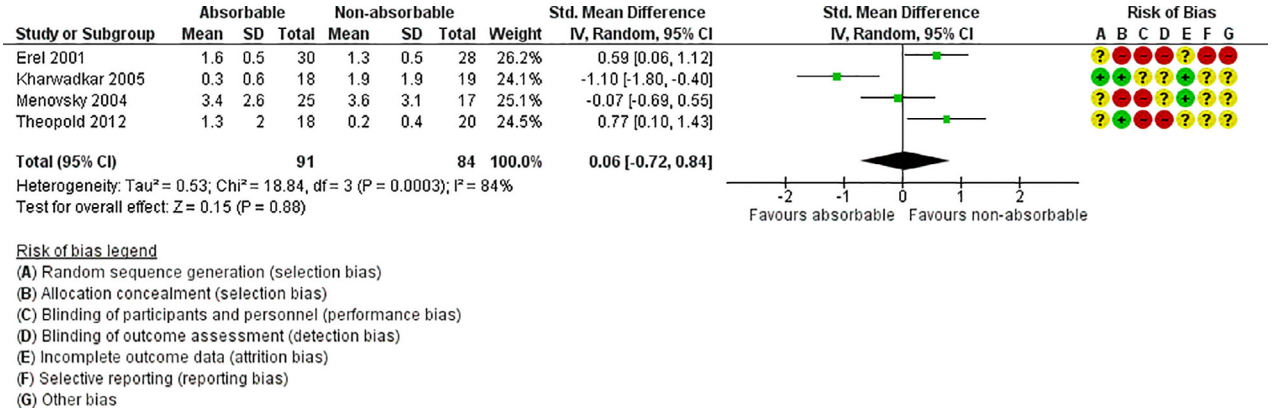

Figure 2 A forest plot of patient-reported pain 6 weeks following open CTD, showing no evidence of a difference between absorbable and non-absorbable sutures.

Conflicts of interest

There are no conflicts of interest.

Ethical review

Ethical review was not required as this is a review of published literature. Further, this article does not contain any studies with human participants, which were originally performed by any of the authors.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.bjps.2018.08.006.

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Endoscopic assisted retrograde release of posterior interosseous nerve: Preliminary results of a new technique☆

Dear Sir,

Introduction

Radial tunnel syndrome (RTS) is compression of posterior interosseous nerve (PIN) causing pain on proximal of dorsal forearm.[1] The cause of RTS is compression of PIN on forearm

☆ The study presented as an oral presentation at 16th National Congress of the Turkish Hand and Upper Extremity Society at 6–9 May 2018.

by one or more of 5 following structures: (1) fibrous bands above radial head, (2) branches of radial recurrent artery (leash of Henry), (3) tendinous edge of extensor carpi radialis brevis(ECRB), (4) proximal fibrous border of supinator >(arcade of Frohse) (most common compression site), (5) distal edge of superficial supinator head.[2-4]

Clinical presentation of RTS is commonly pain on dorsolateral of proximal forearm. Initial treatment of RTS is conservative; such as rest, splinting, activity modification, non-steroid anti-inflammatory drugs or steroid injection.[1,3] Surgical treatment should be considered in patients with persistent pain despite 3 months conservative treatment.[1,3]

In literature, surgical treatment of RTS with open approaches has been well described in clinical and cadaveric studies.[4] To best our knowledge, retrograde endoscopic release of PIN has not been reported previously.

The aim of this study was to present preliminary results of endoscopic assisted decompression of RTS in patients with concomitant lateral epicondylitis which was also treated arthroscopically in same session.

Methods

From 2015 to 2017, prospectively collected data of patients who treated by endoscopic-assisted surgery for RTS concomitant with lateral epicondylitis (LE) were analyzed retrospectively. The patients have been given detailed information about the study, the written informed consent was obtained from all participants This study was conducted in accordance with the rules of the Declaration of Helsinki. It was approved by the Ethics Committee of the University, Faculty of Medicine. Operative treatment was considered for patients did not respond to non-operative treatment. Exclusion criteria's were traumatic injury history of elbow, patients with inflammatory tenosnynovitis, patients with other symptomatic diseases at same limb such as frozen shoulder.

Surgical technique

All patients operated under infraclavicular block or general anesthesia with a pneumatic tourniquet placed on proximal arm. Patients arm and forearm placed on a hand table in neutral position. A 2 or 3 cm length longitudinal incision was made 6–8 cm distal to lateral epicondyle in the line with lateral epicondyle and Lister's tubercle (Figure 1(a)). The plane between extensor digitorum communis(EDC) and ECRB was dissected and, PIN exposed at the emerging site of distal supinator with protecting the branches to EDC (Figure 1(b)).

Then, a hooded endoscope with assembled a 4 mm 30° angled scope (KARL STORZ, Tuttlingen, Germany) is inserted and superficial supinator muscle belly dissected through proximally (Figures 1(c) and 2(a)). The arcade of Frohse, tendinous edge of ECRB was released till leash of Henry (Figure 2(b)). Branches of radial recurrent artery was ligated with hemoclips and cut by endoscopic bipolar cautery (Figure 2(c)). Fibrous bands above the radial head was released.

If necessary, release of radial nerve 5–15 cm proximal to distal end of supinator muscle could be done via endoscopic dissection (Figure 2(d)). If the patient had additional nerve compression syndromes (e.g. cubital (CuTS) or carpal tunnel syndrome (CTS)), endoscopic release of compressed nerve was done. Then, tourniquet was released and hemostasis was obtained. The subcutaneous layer and skin was closed with interrupted sutures.

Then patient placed in lateral decubitus position and using standard elbow arthroscopic portals, LE treated arthroscopically. A compressive dressing was applied to elbow and forearm for 2 weeks. Patient allowed to immediate mobilization.

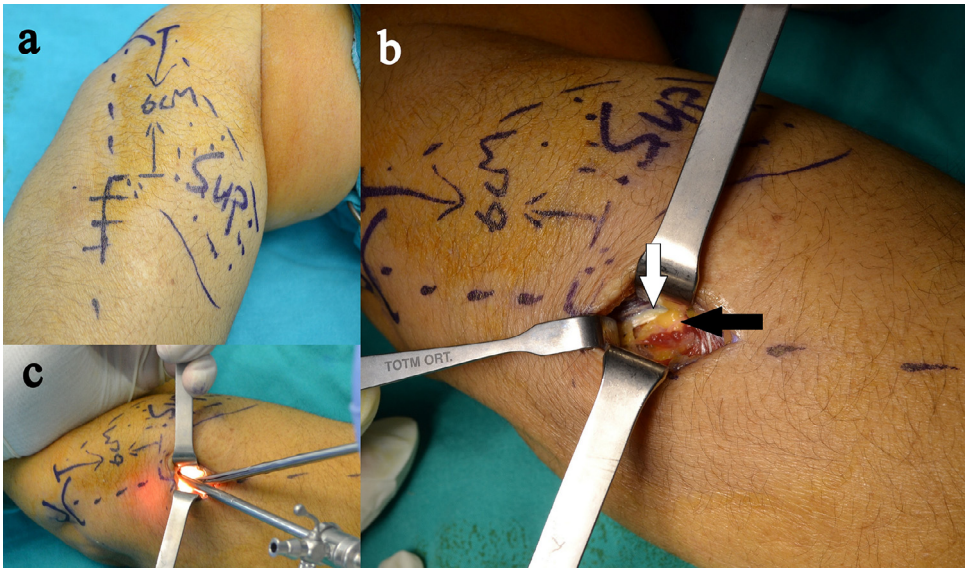

Figure 1 (a) Landmarks of skin incision. (b) Exposure PIN (white arrow: EDC muscle, black arrow: PIN). (c) Insertion of endoscope.

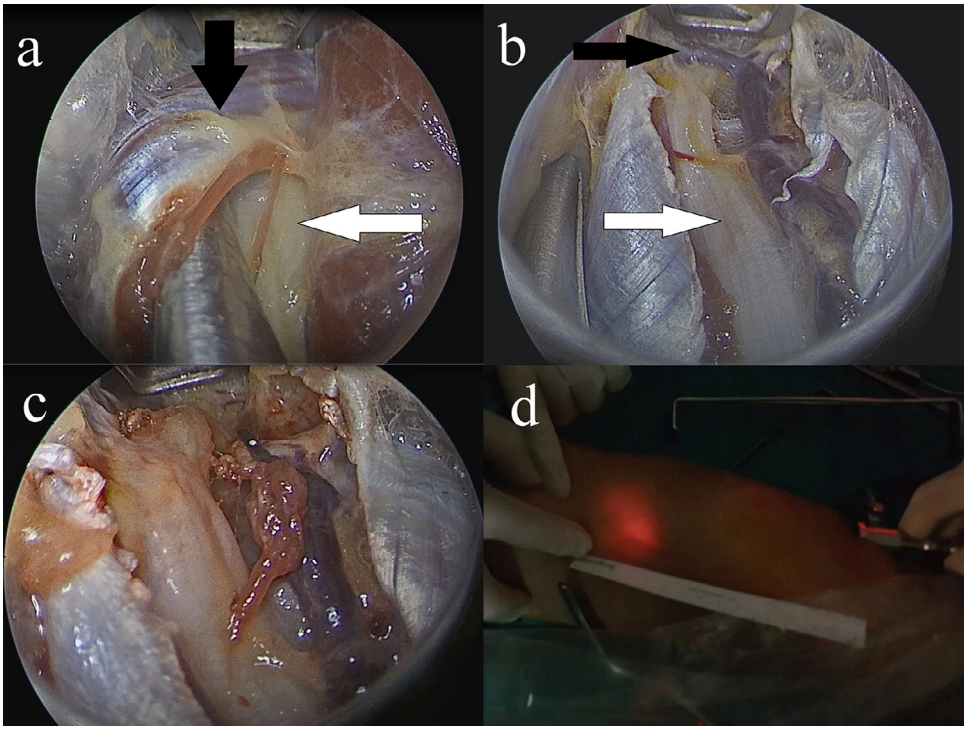

Figure 2 (a) Endoscopic view of supinator muscle (black arrow) and PIN (white arrow). (b) Decompression of PIN till leash of Henry (black arrow) (white arrow: PIN). (c) Ligation and cutting of leash of Henry. (d) Length of release might be done with endoscopic technique.

Results

Ten patients(6 females and 4 male) with concomitant RTS and LE was treated with endoscopic-assisted PIN release and arthroscopic debridement of LE. Three patients had additional CTS and one patient had both CuTS and CTS. In 7 patients, the affected side was right and in 3 patients affected side were left. Mean age of patients was 51.5 years (range, 33–56 years). Mean follow up time was 24,6 months(range, 6–36 months).

In final follow up, using the Roles and Maudsley criteria, 5 patients had excellent result (50%), 3 good result (30%), one fair (10%) and one poor result (10%). DASH and Mayo scores were improved from 50.0 to 6.8 ($p < 0{,}005$), 52.5 to 87.5 ($p < 0{,}009$), respectively. All of patients but one were satisfied from the surgery in terms of the symptom relief and improved functional state.

The complications were an isolated 3th finger extension lag as 30° that recovered satisfactory after a side to side EDC tenodesis procedure and a transient neuropraxia that was fully recovered in 3 months without any intervention.

Discussion

Endoscopically assisted antegrade release of PIN was described by Leclere et al.[5] In their series, Leclere et al. used two incisions, first one to release proximal radial nerve and second one to release PIN. In our technique, it is possible to release radial nerve till 5–15 cm proximal from distal end of supinator muscle (Figure 2).

In conclusion, our study showed that endoscopically assisted RTS release could be a good option for treatment of RTS and is less invasive than open procedures previously reported in literature with similar clinical results and complication rates. Further randomized prospective studies with control group and more number of patients are needed to make validate decision about this technique.

Acknowledgments

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Conflicting interests

None.

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Techniques to enable identification and safe elevation of the posterior interosseous artery flap

Dear Sir,

The distally-based, pedicled posterior interosseous artery (PIA) flap can be a useful option for reconstruction of dorsal hand defects, particularly following trauma.[1,2] The flap is generally thin and pliable and avoids sacrificing a major vessel supplying the hand. It can be raised as a skin flap, adipofascial variant, or the super-thin fascia-only type.

The PIA flap has fallen out of favour with some surgeons due to its supposed complexity in elevation and difficulties identifying the pedicle. Some authors have commented on a high rate of flap congestion (30%).[3] Acharya et al. have described modifications to ensure for a more reliable PIA flap, for example by taking a cutaneous handle with the flap pedicle; in our experience this is unnecessary and compromises the aesthetics of the reconstruction.[4] It may also increase the difficulty of transposing and insetting the flap; we find the slim but robust pedicle a distinct advantage in dorsal hand reconstruction.

We mark the axis of the PIA from the DRUJ to the lateral epicondyle. The distance from the pivot point, just proximal to the DRUJ, to the nearest part of the defect for reconstruction is measured to identify the correct pedicle length.

Dissection starts distally to precisely identify the pedicle location, which lies in the septum between the EDM and ECU compartments. Two simple manoeuvres can be performed distally to help identify the PIA pedicle, which is often small, and can be damaged.

1) The dissection is started suprafascially; small perforating branches will be seen coursing from the skin through the fascia, which arise from the PIA pedicle. Visualisation of these perforators helps determine the position of the pedicle and, therefore, where the surgeon should incise the fascia distally.
2) Passive movement of the little finger causes the EDM tendon to move under the translucent fascia, providing further visual assurance of the position of the relevant septum and therefore the vascular pedicle.

Once the septum between ECU/EDM compartments is identified, an incision is made and the pedicle is identified against the septum. A narrow strip of fascia is maintained over the septum and pedicle, which protects the latter and simplifies elevation of the PIA flap (Figure 1). The position of the PIA skin island may have to be changed after determining the axis of the PIA pedicle distally. These simple steps avoid the risk of inadvertently raising the flap off the line of the vascular axis and therefore missing key perforators (Figure 2), a risk that is increased when using a relatively narrow skin island.

Absence of the anastomotic arc between the AIA to PIA is rare and the senior author does not routinely search for this distally. Some authors have described a proximodistal dissection to identify the main perforator but with this technique one may inadvertedly place the skin paddle off the main perforator and the correct axis of the PIA pedicle.[4]

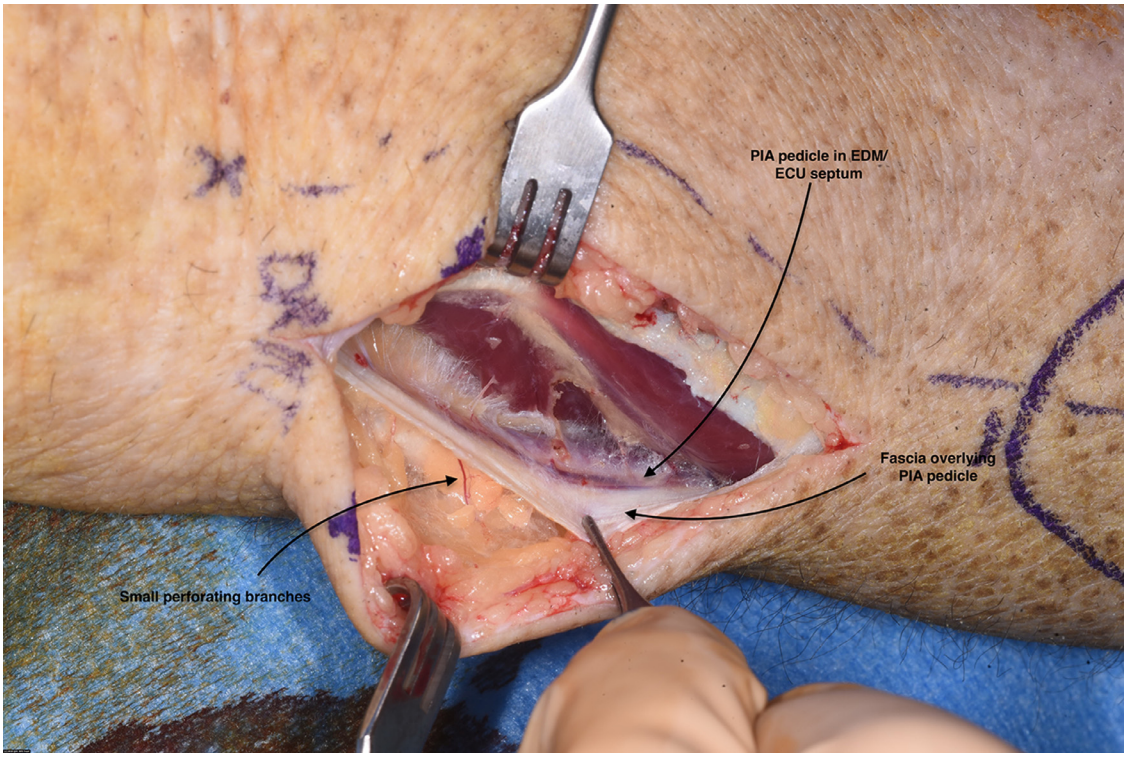

Figure 1 The PIA pedicle lies against the septum between the ECU and EDM extensor compartments. (Distal to the left, proximal to the right.)

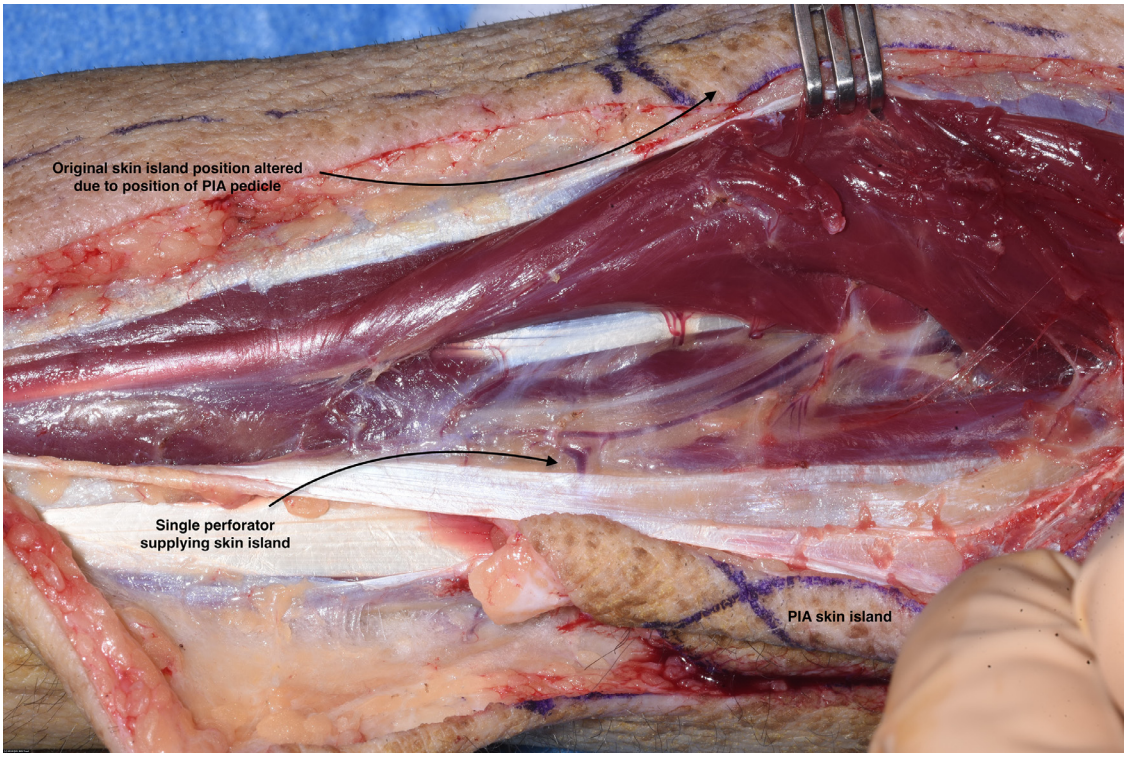

Figure 2 Large perforator shown feeding the skin island. Note: the provisional skin paddle position had to be changed after the distal dissection of the PIA pedicle, an adjustment made after accurate identification of the PIA pedicle.

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Inter-operator variability in the sensitivity of sentinel lymph node biopsy for melanoma

Dear Sir,

Following a diagnosis of melanoma, a negative sentinel lymph node biopsy (SLNB) is reassuring, but a cohort of patients will have false negative results and be left with residual cancer in the sampled basin. With the dawn of adjuvant treatment in sentinel node positive patients (Stage IIIa) the accuracy of the SLNB result will influence survival. False negative results are usually defined as a nodal recurrence following a negative SLNB in the same basin. Rates of false negative results vary in the literature depending on the calculation used, some authors choosing to divide the number of false negatives by total sample size, total number of negative biopsies or the sum of false negatives and true positives. Terms such as sensitivity may avoid confusion, and this ranges from 79–91%.[1]

To ensure accuracy in this diagnostic procedure, trainee breast surgeons in the UK must complete 70 axillary SLNBs under supervision before gaining their certificate of completion of training.[2] This target is supported by evidence that breast surgeons experience a learning curve when starting to perform this operation, with accuracy improving over their first 20 cases.[3] On the other hand, plastic surgery trainees, in some circumstances, can progress to consultant level having performed only 15 generic lymph node surgeries, with SLNB not specifically stipulated on the Joint Committee on Surgical Training (JCST) certification guidelines[2]. The accuracy of SLNB for breast cancer has been shown to be surgeon-dependent[4], and in this study we aimed to investigate whether there was a surgeon-specific variability in the sensitivity of SLNB for melanoma amongst new consultants.

Our centre has a large skin cancer unit and was one of the first in the country to introduce SLNB for melanoma. Today, our independent operators are expected to have well exceeded the indicative numbers for lymph node surgery set out by the JCST. The majority of SLNBs carried out at our centre since 1999 have been performed by eight surgeons, six of whom had been formally trained and mentored in our unit or previously completed skin cancer fellowships. Two surgeons undertook sentinel node biopsy without formal mentored training.

We analysed the first 40 cases performed by each consultant with a null hypothesis that surgeons perform the procedure without inter-operator differences in sensitivity and an alternative hypothesis that those with more extensive training would achieve higher sensitivities. Sensitivity was defined as the number of positive biopsies divided by the sum of the number of positive biopsies and false negatives. False negatives were defined as a clinical nodal recurrence following a negative SLNB in the same nodal basin.

We studied 320 SLNBs performed on 297 patients (with some participants having the procedure performed in multiple nodal basins). Of these 320, 61 were positive and 259 were recorded as negative. After a mean follow up of 6.6 years, six cases were found to be false negatives. The two surgeons who had started practising without formal training had a combined sensitivity of 85% (80 cases, 17 positive results, 3 false negative results). The six surgeons who had been practising after extensive training had a combined sensitivity of 94% (240 cases, 44 positive results, 3 false negative results). SLNBs in the head and neck region accounted for 17% of total cases, but 50% of false positive results. With a total of six false negative results, further multivariate statistical analysis was not appropriate, as 10 false negatives per covariate is recommended for such analyses.[5]

In this study, the two surgeons without formal training attained as many false negative results in their first 40 cases as the other six surgeons combined. There is likely to be a learning curve for this operation, as described in SLNB for breast cancer.[3] Due to the importance of the SLNB result in managing patients with melanoma we suggest that the JCST should stipulate sentinel node biopsy as a specific procedure on the certification checklist. For surgeons specialising in skin cancer surgery, we advocate a similar level of experience to that required of breast surgeons before performing the procedure independently.

Head and neck sentinel node biopsy in our unit is now only carried out by specialist head and neck plastic surgeons, specifically trained in this area, who form part of skin and head and neck multi-disciplinary teams.

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Conflict of interests

Nil

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The use of pulse oximetry to diagnose limb ischaemia

Dear Sir,

The accurate diagnosis of limb ischaemia in the acute trauma setting is time critical. In cases of polytrauma and complex limb injuries this assessment can be difficult and is often first performed by a junior member of the team. The assessment of neurovascular status of these injuries is much the same as it was decades ago. Recently clinical methods thought to be of use are being proven otherwise. In our trauma centre we now routinely use pulse oximetry in the assessment of limb and digit ischaemia.

The salvage rate for ischaemic limbs reduces with time, making early recognition crucial. Successful revascularisation falls from 88% to 61% after 6 h post injury. Angiography has a role, but data suggest that this process often causes delay without demonstrating an appreciable advantage for limb salvage.[1]

Guidance is available from the combined British Orthopaedic Association and BAPRAS Open Fractures of the Lower Limb, and from NICE pathways for Trauma.[2,3] The 'hard signs' suggesting vascular injury are loss of palpable pulses, continued blood loss and expanding haematoma. Palpating peripheral pulses with tissue swelling, hypotension or arterial vasospasm may be difficult however whilst BOA/BAPRAS suggest using Doppler ultrasound to overcome this, NICE specifically recommend not to rely on a Doppler signal to exclude vascular injury. Neither party advocate the use of distal capillary refill time, particularly when pulses are impalpable.

Pulse oximeters measure tissue oxygen saturation by measuring the amount and wavelength of light absorbed whilst passing from one side of the digit to the other, a technique known as spectrophotometry. The accuracy of readings may be confounded by technical issues (nail varnish, strong ambient light, and choice of probe), or patient factors (hypotension, cold limbs, probe away from heart level). The measurement is continuous and therefore produces a dynamic waveform throughout pulsatile blood flow. The standard waveform is a sharp upstroke during peak systole followed by a prolonged downstroke with a dicrotic notch towards the baseline.

There is good evidence of use in the assessment of fingertip injuries which shows that an SpO2 of $>$ 95% has a 100% positive predictive value for vascular integrity.[4] Similar information was available as far back as the 1980's demonstrated by Graham et al[5] for replantation surgery, and there is good reason to believe or at least investigate the translational value in more significant tissue volumes. Furthermore, to supplement the SpO2 reading, the waveform produced by pulsatile flow through tissues detected may provide further insight into limb perfusion.

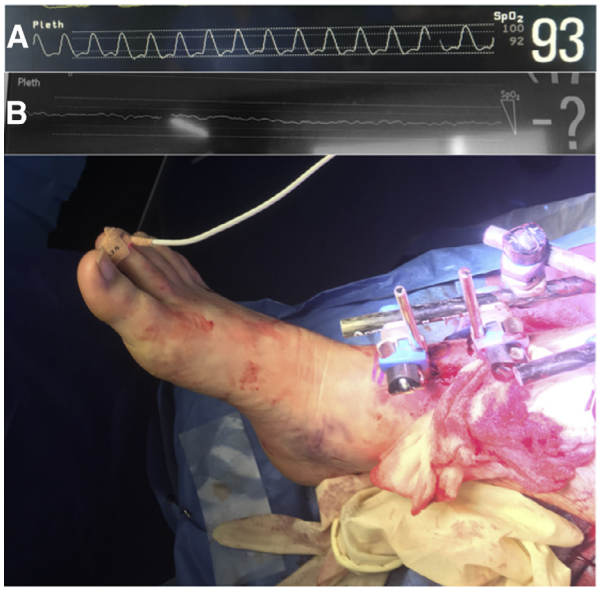

Figure 1 A: Normal pulse oximetry waveform, B: Abnormal waveform from lower limb with single vessel injury.

We routinely use pulse oximeters with graphical waveform displays in our emergency department, as well as using sterile low profile probes intra-operatively (Figure 1). In a limb with transection of all axial arteries no waveform will be present, however a numerical reading may still measure, which will be significantly lower than the central saturation value. This often corresponds to limbs where some degree of capillary refill may be present and highlights the subjectivity of capillary refill as an assessment tool in limb ischaemia. In limbs which are still distally perfused but one of the axial vessels has been transected we have found that the saturation reading is the same as centrally, however the waveform is clearly abnormal. It often has a dampened appearance with loss of triphasic flow when compared to an uninjured limb or centrally. More recently we have been using continuous pulse oximetry as the only method of nursing post-operative monitoring of both micro and macro replants. (Figure 2)

The accurate and timely diagnosis of limb ischaemia is critical to achieving the best outcomes for these patients. Pulse oximetry is an example of a simple and accessible technology which has potential for implementation in the assessment of limb injuries. Although our experience thus far is anecdotal, a prospective observational study is underway to better understand how this technology might supplement current clinical methods.

Conflict of interest

None

Funding

None

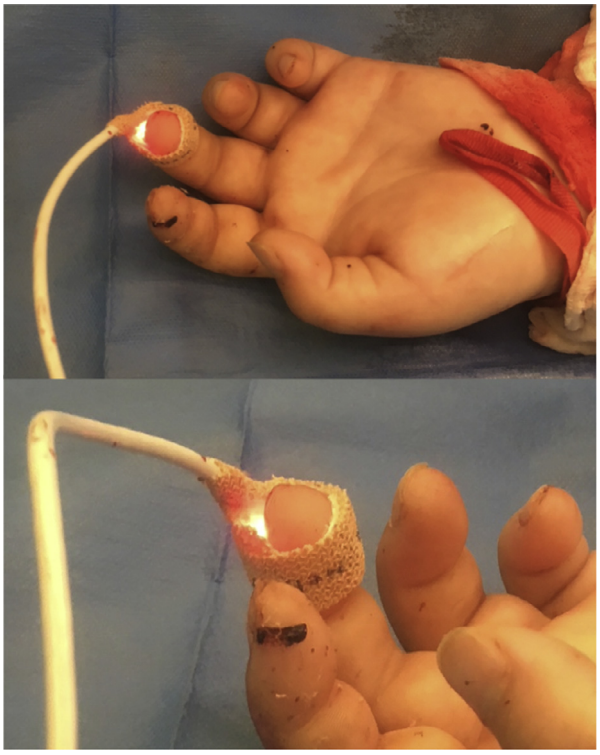

Figure 2 Paediatric Elastoplast pulse oximeter.

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Half-transection method combined with intravascular stent for supermicrosurgical lymphaticovenular anastomosis☆

Dear Sir,

Since the revolution of supermicrosurgical technique was first published in 2000 as the least invasive treatment for lymphedema, lymphaticovenular anastomosis (LVA) has become popular for the treatment of lymphedema and has been proved effective in reducing lymphedema severity.[1-3] However, the lymphaticovenular anastomosis is a demanding technique because the functioning drainage lymphatic vessel is translucent, soft and has a thin wall. To overcome these challenges, the Intravascular Stent (IVaS) has been applied for safe and precise LVA.[4] Even so, the completely transected lymphatic vessel, which is prepared for end-to-end LVA, always shrinks and its lumen is hard to identify especially when the diameter is smaller than 0.5 mm. In this situation, the attempt to insert the stent into the shrunken lymphatic vessel becomes troublesome. To address this difficulty, we devised a "half-transection" method, in which the stent could be easily inserted into the lymphatic vessel, to simplify the anastomosis.

A drainage lymphatic vessel and a recipient venule are prepared as previously reported.[3] First, we transect the recipient venule for an end-to-end anastomosis. Second, the drainage lymphatic vessel which is marked by sterilized blue pen is cut off at the site as proximal as possible and a half width of the lymphatic vessel is incised about 1 mm distal to the cut end. After that, we use the supermicrosurgical forceps to hold the distal end of the lymphatic vessel and stretch it gently towards the proximal direction to open the half-width incision on the lymphatic vessel (Figure 1). In this way, there will be an open window on the sidewall of the lymphatic vessel which is guided clearly by the colorful contrast between the blue half-transected wall and the white inner lumen. Then, we could insert the 8-0 nylon monofilament stent easily into the lymphatic vessel through this window (Figure 2). The other end of the stent could be inserted into the venule or another thicker stent could be used for the venule depending on its diameter. With the appropriate traction of the end of the lymphatic vessel and the guidance of the stent, we could perform the end-to-end anastomosis quickly and safely with 12-0 nylon thread. After the anastomosis of the open half of the lymphatic vessel with the venule, the other half of the lymphatic vessel is transected completely and the anastomosis continued. Before the last stitch is tied, the stent is taken out and the anastomosis is finished. The youngest patient for whom this method has been used was an 11-month-old boy with primary lymphedema of the right leg. He has undergone 4 LVAs by the half-transection method with all the lymphatic vessels and venules thinner than 0.5 mm. The thinnest lymphatic vessel anastomosed was 0.25 mm in diameter and

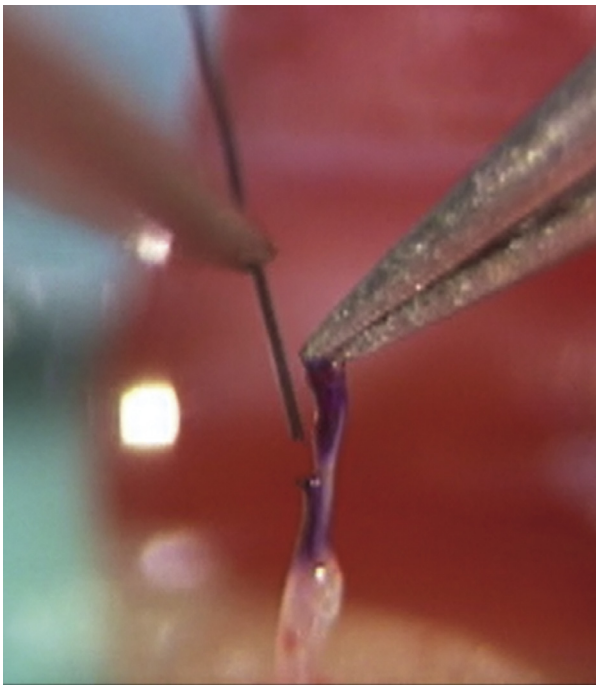

Figure 1 Trying to insert the stent into the lymphatic vessel through the half-transected hole which has been opened under the appropriate traction of the end of the lymphatic vessel.

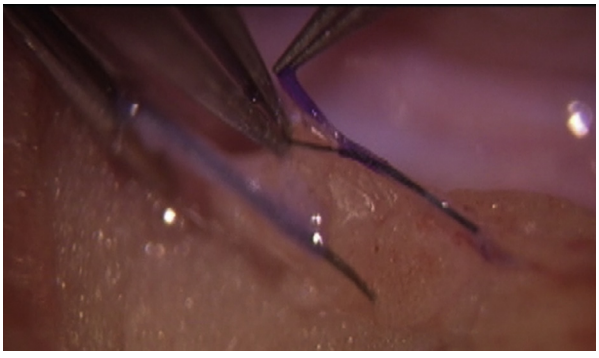

Figure 2 The stent has been inserted into the lymphatic vessel through the half-transected wall, after which the anastomsis could be performed under the guidance of the stent and the appropriate traction of the end of the lymphatic vessel.

☆ Prior Presentations:None.

the thinnest venule 0.3 mm in diameter. According to this method, even less-experienced microsurgeons can perform LVA smoothly with very thin lymphatic vessels. The half-transection method together with Intravascular Stent facilitates lymphaticovenular anastomosis and could be useful for microsurgeons to accomplish LVA efficiently.

Disclaimers and disclosure of conflicts of interest

None.

Sources of support that require acknowledgement

None.

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The amounts of melanin pigment causing color differences between the vermilion and lip mucosa

Dear Sir,

The vermilion, also called red lip, is a rare and limited tissue with red-colored skin. Therefore, lip reconstruction with large tissue defects including the vermilion is generally difficult and challenging. The lip mucosa is often used to reconstruct the vermilion due to its color match.[1] However, the slight difference in tone of the red color between the original vermilion and the transplanted lip mucosa remains postoperatively, and also remains in the case of aberrant lip mucosa after burn injury.

Various colors in human skin are made by the differences of types and amounts of melanin pigment synthesized by melanocytes in the epithelial basal layer.[2,3] We hypothesized that the differences in number of melanocytes and amount of melanin pigment between the vermilion and lip mucosa are responsible for the difference in red color between the two tissues. This study was performed to examine whether the numbers of melanocytes or amounts of melanin pigment in the vermilion can be varied to that in the lip mucosa. To evaluate the numbers of melanocytes and amounts of melanin pigment, we performed hematoxylin and eosin staining, Melan-A immunostaining and Fontana-Masson staining of lip tissues, including the vermilion and lip mucosa, from patients with cleft lip undergoing primary nasolabial repair.

Histologically, no clear border was observed between the vermilion and lip mucosa, and the type of epithelial tissue changed gradually. There were no cutaneous appendages, such as sebaceous glands or hair follicles, in the vermilion as compared with white lip. The lip mucosa also had no cutaneous appendages but contained salivary glands. The vermilion and lip mucosa had muscular tissue, i.e., the orbicularis oris muscle from just under the basal layer to the deep layer.

On immunostaining, Melan-A-positive cells were observed in the basal layer of the lip epidermis and hair bulbs extending dendrites. The numbers of Melan-A-positive cells in the vermilion were much higher than in the lip mucosa. The mean percentages of Melan-A-positive cells per unit area were $2.55\% \pm 1.13\%$ for vermilion and $0.94\% \pm 0.28\%$ for the lip mucosa. This difference in Melan-A-positive cells per unit area between the vermilion and lip mucosa was statistically significant ($p = 0.011$).

Next, we performed Fontana-Masson staining to detect melanin pigment in lip tissue, especially the vermilion and lip mucosa (Figure 1). In the vermilion, Fontana-Masson stain-positive cells were located just above and close to the basal layer. Compared to Melan-A immunostaining, there was almost no positive reactivity in the lip mucosa. The mean percentages of areas of melanin pigment per unit area were $0.93\% \pm 0.58\%$ for the vermilion and $0.12\% \pm 0.06\%$ for

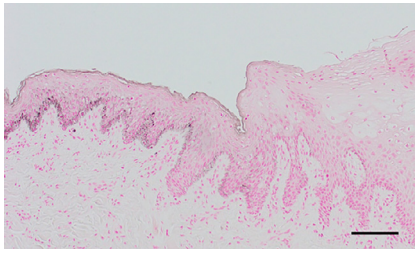

Figure 1 Fontana-Masson staining in lip tissue (scale bar = 100 μm). Fontana-Masson stain-positive cells were presented at the basal layer of epithelium in vermilion (left side) but barely in lip mucosa (right side).

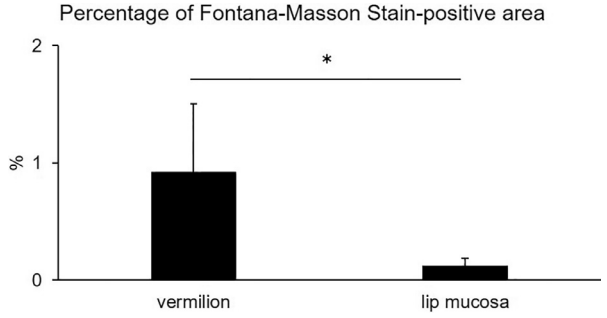

Figure 2 Statistical analysis of Fontana-Masson stain in vermilion and lip mucosa. The vermilion contained 7.8 times as much melanin pigment than the lip mucosa. $^{*}p < 0.05$.

the lip mucosa; the difference was statistically significant ($p = 0.002$) (Figure 2).

Small numbers of melanocytes and small amounts of melanin pigment were present in the lip mucosa, the vermillion contained 2.7 times as many melanocytes and 7.8 times as much melanin pigment than the lip mucosa. These results indicated that the lip mucosa presented a more vivid red color reflecting deeper tissues, such as blood and muscles, because of the small amount of melanin pigment. On the other hand, the vermilion was a dull red color due to the large amount of melanin pigment. Differences in color tones are among the post-operative drawbacks after vermilion reconstruction using lip mucosa, which may be affected by the difference in amount of melanin pigment.

The number of melanocytes and amount of melanin pigment in each sample showed large differences among individuals. There were no significant differences with regard to sex, type of cleft lip, or the presence of inflammation. The lip tissues examined in this study were located at the cleft margin, including white lip, vermilion, and lip mucosa. Ectopic Mongolian spots have been reported in cleft lip.[4] However, intradermal melanin pigment, characteristic of ectopic Mongolian spots, was not confirmed histologically in Fontana-Masson staining in our study.

To provide a color improvement in lip mucosa transplantation, it may be effective to activate the small number of melanocytes in the lip mucosa and synthesize melanin pigment. However, techniques to control the amounts of melanin pigment production have not been developed and additional investigations regarding melanogenesis are required.

In this study, we found that the lip mucosa had a small number of melanocytes and small amount of melanin pigment in comparison with the vermilion. This difference in amount of melanin pigment was suggested to be one of the causes of the color difference between the vermilion and the transplanted lip mucosa.

Author contributions

Ikkei Takashimizu: Study design, Data collection, Data analysis, Writing the paper

Shunsuke Yuzuriha: Study design, Data analysis, Revising the paper

Conflict of interest

None.

Acknowledgments

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Re-Using the internal mammary artery as recipient artery in cervicofacial reconstruction by fibular flap

Dear Sir,

We have read with great interest Decaudaveine's anatomical study entitled "Using the internal mammary artery as recipient artery in cervicofacial reconstruction by fibular flap".[1] We congratulate the authors for their interesting and innovative work, opening new perspectives of maxillofacial reconstructive surgery in patients with vessel-depleted neck area. We published last year a quite similar study in *Surgical and Radiologic Anatomy*.[2] This paper was based on a medical thesis defended in our Faculty in 2016. It seemed useful to compare some data to specify possible future clinical applications.

Instead of proceeding through a parasternal approach with costal cartilages resection, we designed our novel protocol with a median sternotomy after having evaluated the opportunity to do so with the cardiovascular surgery team of our Institution. This concept was inspired by the coronary bypass technique routinely used since a long time. This wide opening of the thorax may appear more invasive than the simple resection of some costal cartilages, but seems faster, and would make it easier to reroute of the internal thoracic vessels to the facial region. Indeed, we noted that a contralateral rerouting provided a better vessel angle at the rotation point, more favorable to microsurgery from a rheological point of view.

Our study involved 20 formalin-embalmed anatomical subjects, or 40 arteries (compared to the six arteries of Decaudaveine's paper). Bilateral comparisons were also possible, and we were able to evaluate the best position of the rerouted vessels according to the area to rebuild. The mean length of the internal thoracic artery was 177 mm (versus 120 mm for Decaudaveine), for a distal diameter of 2.36 mm (versus 2.75 mm). In comparison, and according to the literature, the mean diameters of the arteries in antebrachial, *latissimus dorsi* and fibular flaps were 3, 2.75 and 2 mm, respectively. It meaned that a more distal dissection and a more proximal centre of rotation allowed bringing better positioned and more congruent vessels with those of the most commonly used flaps. The length of the pedicle allowed us to reach the mandibular angle at least in 85% of cases; in the other cases, the extremity of the rerouted vessels reached the oral floor, i.e. a significantly higher level than in Decaudaveine's study. This was probably due to a more distal dissection protocol, until the division of the internal thoracic artery, whereas the Decaudaveine's study stopped the dissection at the 5th costal cartilage. What is more, the opening of the thorax allowed a higher rotation point in our study (brachiocephalic vein vs. first rib).

On the other hand, a clinical evaluation of the respiratory consequences of a median sternotomy versus four costal cartilages resection was necessary in case of clinical application of this experimental protocol. The heaviness of the surgical procedure must be related to the benefit provided to patients cured of their cancer, but whose quality of life is strongly altered by the sequelae inaccessible to classical treatments. It was not clear that the ventilatory restrictions linked to a median sternotomy were greater than those related to the resection of four costal cartilages. Preparation for surgery (functional measures, physiotherapy…) would probably be necessary.

Once again, we congratulate the authors for their valuable contribution in the field of complex reconstructive maxillofacial surgery and look for to sharing our future clinical experiences.

Conflict of interest

The authors declare they don't have any conflict of interest.

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Comments on "Long-term outcome of patients with or without osseointegrated implants after resection of mandibular ameloblastoma and reconstruction with vascularized bone graft: Functional assessment and quality of life"

Dear Sir,

We would like to take the opportunity to comment on the recently published article by Pappalardo et al.[1] "Long-term outcome of patients with or without osseointegrated implants after resection of mandibular ameloblastoma and reconstruction with vascularized bone graft: Functional assessment and quality of life". As the authors mentioned, dental rehabilitated patients had a better functional and aesthetic outcome and quality of life. With experiences in our hospital, we indeed agree with these viewpoints. However, we found there might be some mistakes in the reported data, some of which would even lead to different conclusions.

Firstly, there might be evident errors in nondental rehabilitated patients' number. When patients were divided according to radiological pattern and clinicopathological subtype, the total number in nondental rehabilitated group was 11 instead of 12. We assumed the reported *P* value was correct, thus the possibly true data were listed in Table 1.

Secondly, there might be mistakes in *P* values of Oral health impact profile-14 (OHIP-14) questionnaire scores. We could not get the reported *P* values by either one-tailed or two-tailed student *t* test (Table 2). And if our calculation was right, the difference of psychological discomfort would not have reached the significant level as the study showed.

Thirdly, after repeating calculation of *P* values for continuous data, we found that authors adopted one-tailed student *t* test when calculating *P* values of functional, aesthetic and quality-of-life outcomes, which was not especially pointed out in the method section. As we all know, compared with two-tailed student *t* test, it is easier for one-tailed student *t* test to reach a statistically significant level and obtain the results of significance. We must cautiously apply one-tailed test with a strict indication, whereas in this article, extensive application of one-side student *t* test without clear explanation might be not appropriate[2]. As Hurlbert[3] pointed out that One-tailed tests are rarely appropriate in either basic or applied research. Therefore, we recommend authors not to apply one-tailed test in this article. And if so, difference of anxiety in University of Washington quality of life (UW-QOL) questionnaire scores would not have reached a significant level (two-tailed *t*' test would be applied due to heterogeneity of variance).

To summarize, there were some mistaken data in this article, and the statistical method applied for continuous data might be not appropriate. If data corrected or method changed, some difference would not be significant anymore. These mistakes led to a decrease of readers' trust to the credibility of data. We would like to invite authors to report the correct data by corrigendum.

Table 1 Corrected patients' number according to the reported *P* value in patient and tumor characteristics.

	Dental rehabilitated	Nondental rehabilitated	*P* value
Radiological pattern			
Unilocular	8	3	0.7
Multilocular	14	9[a]	
Clinicopathological subtype			
Unicystic	6	4[a]	0.71
Solid/Multicystic	16	8	

[a] Reported values were one less than the corrected values.

Table 2 Calculated *P* values according to reported mean and SD values in OHIP-14 questionnaire scores.

Domain	Reported *P* value	Calculated *P* value	
		One-tailed	Two-tailed
Functional limitation	0.19	0.26	0.53
Physical pain	0.05	0.12	0.25
Psychological discomfort	0.02	0.08[a]	0.16
Physical disability	<0.01	0.03	0.05
Psychological disability	0.26	0.32	0.65
Social disability	0.06	0.14	0.28
Handicap	0.07	0.14	0.29

[a] The conclusion would be altered if calculated *P* value was true.

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Re: Wound outcomes in negative pressure dressings (WOUND) study – A randomised trial in lower limb skin cancer grafts

Dear Sir,

We read with great interest the paper by Vather et al[1]. It has been well established in the literature for over 20 years that early mobilisation after skin grafting of lower limb wounds does not appreciably harm graft healing[2-4]. The traditional bolster dressings and five days of bed rest, is of largely historical interest and is far from the standard of care in most UK plastic surgery units. A more pertinent question is whether topical negative pressure (TNP) dressings can demonstrate superiority over the true standard of care in most units: conventional circumferential bandaging, immediate mobilisation, and same-day discharge home. Given the greater cost and inconvenience of TNP dressings, it is incumbent on the investigators to establish some significant advantage, rather than demonstrate its equivalence with the far cheaper and more user-friendly status quo. Application of TNP can increase operative time, be cumbersome for the patient to mobilise with, pose a trip hazard for the elderly, and be more time consuming to remove. Small, portable TNP devices may mitigate for some of these drawbacks but are expensive compared to simple dressings.

A more instructive study design would have compared conventional dressings with TNP when both groups are allowed to immediately mobilise and be discharged home on the same day.

Conflict of interest

Nil

Funding

Nil

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Letter responds to comment on published paper: "Using the internal mammary artery as recipient artery in cervicofacial reconstruction by fibular flap"

Dear Sir,

We have received the letter of Adnot about our study entitled "Using the internal mammary artery as recipient artery inervicofacial reconstruction by fibular flap"[1] and read it with great interest.

They have discussed several items, outlined in their study.[2] In particular, they have described the use of a median sternotomy approach to harvest the internal mammary artery. Indeed they have obtained better results in terms of vascular length (177 mm versus 120 mm in our study). We have chosen to perform our study with a parasternal approach because it was already described in other previous articles on cervical reconstruction using internal thoracic vessels.[3-6] The use of a median sternotomy still seems

more at risk for the patient after surgery, with the known complications such as pain, sternal wound dehiscence or infection,[7] and ventilatory restriction. The ventilatory consequences of a parasternal approach are not precisely described in the articles about harvesting the internal mammary vessels for cervicofacial reconstruction, but in all these articles,[3-6] there were no complication on the donor vascular site. Besides, the technique described in Morel's study implies a continuous skin incision from the xiphoid process to the neck, through the tracheotomy orifice. This might have a negative impact on sternal wound healing, with an increased risk of infection. With the parasternal incision, the rotation point is lower (first rib) but the thoracic, cervical and tracheotomy skin incisions are distincts. In case of patients with significant comorbidities, which is always the case in this field of reconstruction, maybe it would be better to use the less possible invasive procedures. Finally the surgical technique of median sternotomy is not routinely used by maxillofacial and plastic surgeons, and requires the presence of a cardiothoracic surgical team, which is not available in all centers, whereas the parasternal approach have been described for a long time in breast reconstruction by plastic surgeons.

Yet, thanks to their innovative approach, Morel's team obtained an increased length for the internal thoracic artery (177 mm), with a correct diameter (2.36 mm), but our results seemed compatible with our goal: anastomosis between the internal thoracic artery and fibular artery in case of mandibular reconstruction with a fibular flap. Indeed in our study, the internal thoracic artery rose 7 mm above the hyoid bone and we considered that the fibular pedicle can reach the hyoid bone level, so the anastomosis should be possible.

However, we are aware this is a small series and that we should continue our work on anatomical subjects before performing the technique to patients.

Thus, this technique has showed encouraging results in both surgical approaches and it is pleasing to debate with a team who have worked on the same topic. We would be glad to share ideas on maxillofacial complex reconstruction in the future.

Conflict of interest

We declare to have no conflict of interest.

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Reply to: Letter to the editor: Evaluation of anatomical and round breast implant aesthetics and preferences in Dutch young lay and plastic surgeon cohort

By Fengrui Cheng; Ying Cen

Dear Sir,

We would like to thank Dr. Cheng and Dr. Cen for reading our article and for submitting their interesting points.[1] All patients included in our study underwent primary aesthetic breast augmentation with either anatomical or round breast implants.[2] Dr. Cheng and Dr. Cen correctly mention that a history of breast disease or surgery would have an impact on breast aesthetics due to skin compliance and breast parenchyma. For this reason, we excluded patients that underwent prior breast surgery as it may have possibly lead to malformation, which could be a reason for revision surgery.

Patient age and implant volume were compared between the anatomical and round implant groups to show that both cohorts matched and that there we no discrepancies, this can be observed in Table 1 (see original article). Breast augmented with round implants were indeed rated more natural by lay participants than surgeons (2.6 ± 1.0 versus 2.2 ± 0.9, $p < 0.001$). However, upon revising our statistical analyses we found that lay participants and surgeons rated results achieved with anatomical implants indeed equally natural (3.3 ± 1.0 versus 3.3 ± 1.0, $p = 0.897$). This is a mistake from our part and should be corrected. We sincerely thank Dr. Cheng and Dr. Cen for inquiring on this matter; yet believe it is noteworthy to mention that this does not change our overall findings.

Continuing, we agree that this topic is still controversial and more high-quality studies are needed to expand our knowledge on the aesthetic outcomes that can be achieved with both implant types. The groundbreaking RCT by Hidalgo et al. provided an excellent start for new similar research on the aesthetic differences between the anatomical and round implants.[3] Although our study differed on several aspect as patients underwent primary aesthetic breast augmentation without initial placement of a different implant. We have mentioned in our discussion that surgeons must be careful breaching the anatomical pocket in which the implant is placed as this could facilitate implant rotation but may also influence short-term aesthetics. Moreover, it is believed that final cosmetic result is best observed after at least 12 months, which we can reaffirm by our own clinical experience. Patients included in our study had their post-operative photographs taken at 12 months, leaving sufficient time for healing and tissue adaption (e.g. capsular formation).

We believe that opinions of plastic surgeons still seem rather polarized resulting in the utilization of only one implant type instead of exploring what kind of different outcomes may be achieved with both. Moreover, a plethora of factors may be analyzed that could influence the perception of aesthetics acquired with anatomical and round implants. The article by Cárdenas-Camarena et al., that was included in our discussion, did focus on several of these factors.[4] For example, one may expect that it is logical for a breast with small volume to obtain the shape of an implant more so than a breast which is covered by more overlying tissue therefore covering the shape.

Finally, we speculate that the most ideal implant probably is a rather smooth-micro-textured, pliable, round implant that feels as soft as normal breast tissue, has minimal risk of capsular contracture, adapts its form depending on patient position and has a dynamic behavior, similar to normal breast tissue during motion (e.g., sporting, running, intercourse): such an anatomically dynamic round breast implants should be the next innovation and relieves us from the discussion to choose between round and anatomical implants.[5]

Conflict of interest

None.

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Comment on "How to assess the volume of a DIEP flap using a free online calculator: The DIEP V (volume) method"

Dear Sir,

Razzano et al. developed a simple method to calculate the predicted volume of a DIEP flap for breast reconstruction.[1] They hypothesized that the best representative shape for a DIEP flap was a truncated pyramid. They measured flap thickness using ultrasound (US), and used other variables including flap length and height to calculate the volume of a truncated pyramid-shaped flap. Great value must be given to this study because estimation of DIEP flap volume is crucial in surgical planning and execution for ultimately matching the volume of the breast being reconstructed, and it also can help decrease donor site complications.[2]

Similar to Razzano et al.'s aim, we developed a prediction model for the estimation of DIEP flap weight, DIEP-W, using the paraumbilical flap thickness obtained from CT angiographic images.[2] Also, we created a prediction model using the pinch test at three paraumbilical sites without using radiologic images.[3]

Razzano et al. used US to measure abdominal flap thickness. Although US is a simple and straightforward method for this measurement, the accompanying pressure applied on the skin can influence measurement, as admitted by the authors, and the intra- and inter-observer reliability need to be verified for broader acceptance.

Razzano et al. measured flap thickness at the base (b) and the top (d) of the truncated pyramid. Abdominal flap thickness differs in both the horizontal and vertical planes in the abdomen; the flap is thicker around the umbilicus level than the lower margin of the flap.[4] The thickness (b) and (d) varies according to the vertical location of the measurement, and it remains unclear whether this issue was handled appropriately.

The authors emphasized that the volume instead of the weight of the DIEP flap should be used for calculation because the density of the breast is different from that of abdominal tissue. Because the density of breast tissue varies significantly among patients, we agree that mastectomy specimen weight is not a reliable reference in the immediate reconstruction setting. In our practice, the target breast volume is measured by cross-sectional CT images (CT-volumetry) or 3-D photography, which is performed on the contralateral side to overcome asymmetry issues of any origin.[5] On the other hand, abdominal flap density has little variation among patients. DIEP flap weight can easily be converted to volume by multiplying by 1.12, because the mean density of the abdominal flap has been reported to be 0.89 g/mL, which was similar to Razzano et al.'s result (0.84 g/mL).[2] Furthermore, the issue of DIEP flap density matters less, as slight overcorrection of breast volume is generally desired, so weight figures still can be used without conversion in calculation to match the target breast volume.

In surgical planning, we compare breast volume and DIEP flap volume or weight, and estimate the inset ratio (flap for final inset/entire DIEP flap) preoperatively. If a high inset ratio is anticipated, a maneuver for securing the perfusion of the DIEP flap, such as bipedicle harvest, is planned.[5] Because the flap weight can easily be predicted intraoperatively according to the size of the flap using a smart phone application known as "DIEP-W," we adjust the final design of the DIEP flap to avoid harvesting an unnecessarily large flap.[2] It seems to be beyond the scope of the paper, but it is not clearly elucidated, how the authors' DIEP flap volume estimation helped in preoperative planning and ultimately affected the reconstruction outcome.

In many ways, we share the authors' goals: achievement of a symmetric breast through volumetric DIEP flap planning with minimal donor site morbidity. It is highly commendable that the authors made a notable contribution to this valuable subject by developing a novel, easy, and accurate method, DIEP-V. Although most surgeons still subjectively estimate breast and abdominal flap volume, efforts should be continued to develop objective and practical methods that can match breast volume and help reduce complications of the donor site.

Conflict of interest statement

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